THE ANNALS

OF

ELECTRICITY,

MAGNETISM, & CHEMISTRY;

AND

Guardian of Experimental Science.

CONDUCTED BY

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AND ASSISTED BY GENTLEMEN EMINENT IN THESE DEPARTMENTS OF PHILOSOPHY.

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OCTOBER, 1836.

I. *The Galvanometer.*

AS one of the particular objects of these Annals is to convey practical as well as theoretical information in those sciences they profess to promulgate, it is proposed as a part of our plan to describe such instruments as have been employed in any of the investigations of our authors, as we think may not be generally known to some of our less scientific readers: and, as far as is considered necessary, to give such practical hints respecting the use of those instruments as either our correspondents may communicate to us, or as our own experience justifies. In the present number, the Galvanometer will be frequently spoken of; and as it has been employed in various forms, a description of those which are now in general use may very properly be described in this place.

A Galvanometer, as its name implies, is an instrument for measuring a Galvanic force emanating from a conducting wire which is the transmitting medium of an electric current flowing from the positive to the negative pole of a Galvanic or Voltaic* battery; or it is a measurer of the force of any electric current, from whatever source of excitation it may proceed.

Its action depends entirely upon the principles of Electro-magnetism, which were discovered in 1819, by Professor

* The same instrument is as frequently called by one of these names as by the other: but as it is the invention of Volta, and not of Galvani, and constructed upon principles which Volta discovered it is properly the Voltaic battery.

Œrsted, of Copenhagen; and its indications are deflections of a magnetic needle situated within the sphere of the electro-dynamic action.

To understand the distribution of this force in the ambient medium, let us permit the circular part *c* of fig. 1. to represent a transverse section of any metallic wire transmitting an electric current; and let the direction of this current be from the end *c*, nearest to the spectator, to the remote end, which may be considered at some distance behind the paper. Then the distribution of the force, under these circumstances, would be at right angles to the axis of the wire, in the manner represented by the four right lines with cross heads which touch the circle: the whole surface of the wire being supposed to be surrounded by similar lines of force, as may perhaps be better understood by consulting fig. 2.

In order to familiarize the *modus operandi* of this force on a magnetic needle, let us admit, for the present, that it is itself magnetic; and that our tangential lines in fig. 1 and 2 are the true representatives of a few of the multitudes of virtual elementary magnets which encompass the conducting wire; and that their cross heads represent poles corresponding to that pole of the needle which, in this country, is solicited by the northern parts of the earth.—Thus circumstanced, it is easily perceived that any magnetic needle placed within the sphere of action of this force would be operated on as decidedly, and in precisely the same manner, as one magnet operates on another; and would, if properly suspended, either by a silken film, or on a fine point as *s n* or *s' n'* fig. 1. be placed at right angles to the axis of the conducting wire; with its *north* pole towards the *south* poles of the electro-magnetic lines of force which are *nearest* to it; and, consequently, with its *south* pole towards the *north* poles of those lines.

Now, as the direction of the polar lines of force *above* the wire appear, to a spectator looking at the section, to be opposite to that of the lines of force *below* it, the needles which are exposed to the action of those forces will necessarily take opposite positions; the north pole of the one being directed towards the right, and that of the other towards the left. In a practical point of view, this fact is very important, as it enables us to combine needles in such a manner as that their directive tendency may be nearly neutralized; and at the same time avail ourselves of the electro-magnetic force on both sides of the conducting wire, as will be more particularly pointed out presently. It must be borne in mind, however, that notwithstanding the appearance of the polar arrangement of the electro-magnetic

force being opposite on opposite sides of the wire to a spectator placed exterior to its surface; this arrangement assumes a very different aspect to a spectator supposed to be placed in its axis; who, looking outwards on any side he pleases, will find it in regular and uniform order; one kind of poles being constantly towards his right, and the other kind towards his left.

Construction of the Galvanometer.—The first instrument which bore any of the appearances of the Galvanometers in present use was the *Multiplier* of Professor Schweigger; which was simply a compass box with a copper wire coiled several times round it, whose convolutions were parallel to the meridian line of the card. This instrument being adjusted to the magnetic meridian, its needle would rest in the plane of the coil, by the force of terrestrial magnetism. If, however, the ends of the coil wire were to be connected, one with the positive and the other with the negative poles of a Voltaic battery,* the coil would become a channel for an electric current; and the electro-magnetic force of this current operating on the compass-needle would deflect it out of its natural direction; and after a few oscillations it would take a new stationary position. This new position of the needle would not be at right angles to the plane of the coil; because of its still being under the influence of terrestrial magnetism, as well as under the influence of the electro-magnetic force; but it would form an angle with the meridian line of the compass-card, the extent of which would depend upon the extent of the latter force; or, if you please, upon the intensity of the electro-dynamic action which kept the needle from returning to its terrestrial line of repose.

If now we consider that the electro-magnetic force is equally distributed over every part of the wire forming the coil, it is obvious, that by passing the wire once round the compass-box, directly over and under the meridian line of the card, this force would be brought into play both on the upper and lower sides of the needle: and these two increments of force would conspire, and sustain the needle

* The positive and negative poles of a *compound* Voltaic battery are, respectively, its *zinc* and *copper* ends; and a wire or other conductor, joining those poles, would permit the flow of an electric current *from* the positive to the negative one. When the battery consists only of a *single pair* of plates, the electric current along the conducting wire is *from* the copper to the zinc. An electric current during the discharge of a Leyden jar, is *from* the positive to the negative surface.

farther from the meridian-line than either of them alone would do.

To familiarize the action which tends to the production of this fact, let us suppose the circles *c* and *z*, fig. 3, to represent transverse sections of the upper and lower portions of the wire; and that the source of electric action is a single pair of copper and zinc plates: then accordingly with what we have already shown, the current would flow *from* the end *c* along the wire, (which imagine to be bent over the farther edge of the compass-box, behind the paper) and return by the section *z*. This being understood, the right lines with cross heads will show the distribution of the polar lines of electro-magnetic force, due to the advancing and returning current. The *north* poles of the two portions of those lines which are situated immediately between the upper and lower parts of the wire, will be observed to be directed towards the spectator's right, and all the *south* poles of those lines, towards his left. And as the needle *n s* is placed between the upper and lower portions of the wire, it is influenced, principally, by those conspiring forces; and is sustained *out* of the magnetic meridian by their joint action.

In pursuing this illustration we may now readily imagine that by passing the conducting wire once round the compass-box, the electro-magnetic action on the needle would be double that which would be due to either the upper or lower part of the coil alone. This being admitted, two such coils would he productive of four times the single force, and three coils would multiply the force six times, and so on: and hence the name *multiplier* being given to this instrument. By this means, an electric current, whose force in one strand of wire alone would be too feeble to produce a perceptible motion of the magnetic needle, may easily be detected;—sometimes by deflections of considerable extent.

Instead of employing a compass-box in the construction of a galvanometer, it is now usual to make an open oblong coil of thin copper wire, previously covered with sewing silk, in the manner that bonnet wire is covered. This silken covering is intended as an insulator, to prevent the electric current from passing laterally from one convolution to another. The number of convolutions in each coil varies with the different views of the contrivers, from ten to more than a hundred convolutions. For general purposes perhaps about eighteen or twenty convolutions are quite sufficient. The convolutions are usually packed close together, and the coil fixed vertically on a board; and each extremity of the wire frequently terminates with a small

cup, for the purpose of holding mercury, which is sometimes useful for connecting the instrument in the Voltaic, or other electric circle. The needle may be of any shape the experimenter thinks proper. Perhaps long thin sewing needles, with the points and eyes cut off, answer as well as any. One of these, when magnetized, may be suspended in the centre of the coil, by means of a silken fibre, which passes through an opening in the upper side, as seen in fig. 4. A deep groove is usually cut in the base board, to place the lower edge of the coil in, and a graduated card for measuring the angles of deflection, is pasted to the surface of the board over the groove.

When the electric forces to be examined are but feeble it will be better to employ two needles, with their like poles in opposite directions, as represented in fig. 5. The advantages derived from this arrangement are that, when the needles are of equal power, they will counteract each other's directive tendency, and obey the influence of an exceedingly slight electric action: and in consequence of their positions, with regard to the coil, they are both urged in the same direction, by the conspiring electro-magnetic forces, to which they are exposed. The needles may be inserted in any light substance, as a thin straw, a piece of dry glass, or a narrow slip of card; and sustained by a silken film, as in the figure; this is called the *astatic* needle galvanometer. The angles of deflection are most conveniently read off, when the graduated card is placed on the upper side of the coil, the upper needle serving as the index. The astatic needle is the invention of the late Italian Philosopher, the Chevalier Leopold Nobili.

As the electro-magnetic force is known to decline with an increase of distance from the conducting wire, another important point to be observed in the construction of these instruments when intended either for the detection, or the admeasurement of feeble electro-dynamic action, is, that the upper and lower sides of the coil should be no farther asunder than is necessary for the free motion of the needle, between them; and the upper needle should be adjusted so as to move as close as possible to the coil. To prevent undulations of the air from disturbing the needles, the galvanometer should be covered with a glass shade. Although the appellation *Galvanometer* is usually given to all the various forms of Schweigger's multiplier, many of them are obviously very far from being deserving of so dignified a title; and are of no farther use than that of indicating the mere existency of an electric current. Such instruments have sometimes been called *Galvanoscopes*.

Dr. Ritchie's Torsion Galvanometer, we believe, is not

generally known, and as it possesses some peculiarities, a description of it will, no doubt, be interesting to many of our readers. The description is in the Doctor's own words as we find it in the "*Journal of the Royal Institution of Great Britain, for October*, 1830.

Dr. Ritchie's description and application of a Torsion Galvanometer,*

In a paper which appeared in the first part of the Philosophical transactions for 1830, I investigated the elasticity of threads of glass, and applied that property to the construction of a delicate Galvanometer. The instrument then described, though sufficient for most purposes, requires some modification to adapt it to researches of extreme delicacy. The description of the instrument, in its more perfect state, with a few of its numerous applications, will form the subject of this communication.

For experimental researches in electro-magnetism, it is extremely useful to have constantly at hand, a quantity of copper wire, of different degrees of fineness, coated with sealing wax. The most convenient mode of giving the wire this coating is the following:—Stretch the wire between two supports, heat it gradually, from one end to the other with an iron bar or spirit lamp, and continue rubbing the heated part with a stick of sealing wax; the wire will receive a fine coating, sufficient to prevent metallic contact, when portions of it are pressed together in the construction of any piece of electro-magnetic apparatus.

Take the wire thus coated, heat it slightly to prevent the wax cracking, and form it into a rectangular shape, consisting of six, eight, or ten repitions of the wire, according to the delicacy of the instrument required. The upper side of the rectangle must then have the wire separated into two equal portions, bent round a small cylinder, and then continued straight, so as to leave a circular opening in the middle, about one third of an inch diameter. The use of the circular opening in the upper side is to allow a slender axis, carrying the magnetic needles to pass through it, in order to increase the power of the instrument, and render the compound needle astatic. Portions of a brass tube about an inch long, are to be soldered to the ends of the

* "The *force of Torsion* is the effort made by a thread which has been twisted, to untwist itself, and return to its former state." And was first employed as a counterpoise for measuring feeble electric and magnetic forces by Coulomb. See Dr. Olinthus Gregory's Translation of The Abbe Haüy's Natural Philosophy. Coulomb's Torsion Balance will be described in a future number.

wires forming the rectangle, for the purpose of holding a small quantity of mercury, to render the metallic contact complete. Figs. 6 and 7 exhibit a vertical section of the rectangle, and a horizontal one of its upper side.

The wires, forming the rectangle, are pressed close together and secured by a waxed sewing thread, rolled tightly round them. The rectangle is then fixed in a rectangular box, having the upper side formed of two sliding panes of window glass, for the purpose of shutting up the needle from the agitation of the air. Each pane has a small semicircle cut out of the middle of the edge by means of a round file, so as to leave a circular opening directly above that in the rectangle. Various contrivances for suspending the magnetic needle might be adopted. The following is perhaps the most convenient:—Into a strong wooden sole or base, fix two upright supports, about three feet long.* A small stage at the top, having a divided circle on its upper side, and which may be elevated or depressed at pleasure, completes the frame of the instrument. The stage has two holes of the same size as the supports, and at the same distance, with two small screws passing through its sides opposite the centres of the openings for the purpose of fixing the stage scurely at a proper height. A small cylindrical wooden key or peg, having a small bore in the axis, for the purpose of receiving the end of the glass thread, passes through the centre of the divided circle, and is made to turn easily, without much friction.

After numerous trials, the following appears to me the best mode of preparing the threads of glass, so as to have their extremities somewhat thick and tapering, for the purpose of securing them in the Torsion key, and in the axis which carries the magnetic needles. Take a solid rod of glass, or a piece of clean thermometer tube, having a fine bore, and draw out one of its ends, as in fig. 8. Direct the very point of the flame on the thick portion at *A*, and pull it out, between the two hands, to the proper length. As it is hardly possible to get a thread of glass of the proper length and fineness at the first trial, it will be found necessary to draw several, and select the one best adapted to the purpose.

Two slender darning needles, of the best steel, are then to be selected, the eyes to be broken off, and the ends filed to a point similar to the other ends, and then strongly magnetized in the usual way.† The needles are then to be

* The fibres suspending the magnetic needles of galvanometers are usually about a foot long.—Edit.

† To magnetize a needle of this description, it is only necessary

fixed transversely in a piece of straw or other light substance about an inch long, and at the distance of about half an inch from each other, with their corresponding poles in opposite directions;—the one needle intended to be above the upper side of the rectangle, and the other below it. One end of the glass thread is then to be securely fixed in the end of the straw or light axis, by means of strong cement or sealing wax, whilst the other extremity is fixed in a like manner, in the centre of the Torsion key, A single fibre of silk, having a small weight attached to it, is fixed to the lower end of the axis, and made to pass through a small hole near the lower side of the rectangle, for the purpose of keeping the axis carrying the needles, in the centre of the circular opening in the coil. The upper needle has two pieces of fine straw, several inches long, fixed on its ends, so that the slightest deflection may be readily observed. The extremity of one of the straws is made to oscillate between two upright pieces of glass, to prevent the needle moving over an extensive arc, and thus lengthen the time necessary to complete an observation. The whole will be obvious from the simple inspection of fig. 9, in which *A B* is the rectangular coil of wire, ; *N S*, *S' N'*, the magnetic needles; *C*, the stage with the divided circle and Torsion key; and *G*. the glass thread. If instead of the glass thread, the needle be suspended by a single fibre of silk, the instrument becomes a galvanoscope of extreme delicacy. The following experiment affords a striking illustration of the extreme sensibility of the instrument with this modification.

Experiment 1. File off a few grains from a piece of zinc and copper, by means of a coarse file; place two of these near each other, in the bottom of a clean watch glass, bring the clean ends of two fine copper wires, connected with the cups of the galvanometer in contact with them, and then drop over them a small quantity of dilute acid, and the compound needle will be deflected several degrees.*

to draw it eight or ten times over the pole of either a bar or horse-shoe magnet, from its centre to one of its points. Then turn the needle in the hand, and draw its other half in a similar manner over the other pole of the magnet. The points of the needle, and those poles which magnetized them, will then be respectively in opposite magnetic states. In a future number of this work, we shall have occasion to speak more particularly of the manner of magnetizing steel needles, and of making artificial magnets of other descriptions.—EDIT.

* An elegant, and perhaps the most accurate method of conducting a *test experiment*, for ascertaining the delicacy of a galvanoscope is to employ cylindrical metallic wires, copper and zinc for instance, of small dimensions, and immerse them to some standard depth, in

The instrument by which I ascertained the existence of a voltaic current from this elementary battery, consisted of a greater number of coils in the rectangle, and the needles were light and strongly magnetized.

Galvanometers used by Dr. Faraday.

Two Galvanometers which have been employed in a great variety of important experiments, are described as below, in the 87th. and 205th. articles of Dr. Faraday's "Experimental researches in Electricity." Phil. Trans. for 1832.

'Article 87. "The Galvanometer was roughly made, yet sufficiently delicate in its indications. The wire was of copper covered with silk, and made sixteen or eighteen convolutions. Two sewing needles were magnetised and passed through a piece of dried grass parallel to each other, but in opposite directions, and about half an inch apart; this system was suspended by a fibre of unspun silk, so that the lower needle should be between the convolutions of the multiplier; and the upper above them. The latter was by much the most powerful magnet, and gave terrestrial direction to the whole. Fig. 10 represents the direction of the wire and the needles when the instrument was placed in the magnetic meridian; the ends of the wire are marked *A* and *B*, for convenient reference hereafter. The letters *S* and *N* denote the south and north ends of the needle when affected merely by terrestrial magnetism; the end *N* is therefore the marked pole. The whole instrument was protected by a glass jar."

Article 205. "To obtain perfectly satisfactory results, a new Galvanometer was constructed, consisting of two independent coils, each containing eighteen feet of silked copper wire. These coils were exactly alike in shape, and number of turns, and were fixed side by side with a small interval between them, in which a double needle could be hung by a fibre of silk exactly as in the former instrument (87). The coils may be distinguished by the letters *K L*, and when electrical currents were sent through them in the same direction they acted upon the needle with the sum of their powers, when in opposite directions, with the difference of their powers."

distilled or in fresh rain water. The electric current from such a standard source of excitation, being made to traverse the coils of different galvanoscopes, would readily detect the different degrees of sensibility of these instruments. A moderately delicate astatic galvanoscope will show a deflection of five or more degrees, by the electro-dynamic action of two such wires, one-twentieth of an inch diameter, having two inches of their lengths immersed in fresh rain water, for two seconds of time.—EDIT.

II. *Description of an unusually large Galvanometer, and of a powerful Electro-Magnet, &c., by* Robert Hare, *M. D. Professor of Chemistry, in the University of Pensylvania ; in a letter to* Mr. Sturgeon.

Sir,

A work of yours, entitled "*Recent experimental researches in Electro-magnetism, &c.*" has become partially known to me through an imperfect copy lent by a friend. This copy extends only to the 84th. page, and does not comprise your theoretical views on the nature and developement of the energies of the Electric Column, &c. &c. which I am desirous of seeing, having been engaged lately in constructing series of that kind. I will take it as a favor if you will send me a perfect copy of the work in question, which, as far as I have studied it, has interested me greatly.*

I have lately constructed some multipliers or Galvanometers of a larger size than I believe to have been used in Europe. The needles are seventeen and a half inches long. I have made a needle of this size vibrate ten degrees nearly, by the contact of a disc of copper of less than an inch diameter with a disc of zinc of the same dimensions, no other excitement being used than pure water. I find that tin foil wound up alternately with thin paper, so as to take the place of the coil of wire covered with silk as usually employed, answers very well; but in the most sensible of my instruments, I have copper wire, No. 16, separated at the corners by thin paper, and well varnished with shell lac :—168 feet of wire.

As soon as I heard of the wonderful magnet of Professor Henry, I repeated his experiments with copper wire varnished as above described; and I have recently made a magnet by means of copper wire, shell lac varnish, and paper surrounding the iron, which in proportion to its weight, holds more than his. It weighs seventeen pounds, and has held seven hundred and eighty-three pounds. It is furnished with fourteen coils of sixty feet each.

* The work in question was proposed to be published in two parts; the first of which appeared in 1830, and was that which Dr. Hare had seen. Shortly after this time, and before the second part could be got to press, the Printer, Mr. Brown, died; and by some means, the manuscript was lost, or at least the author could never recover it, though frequently asked for. Under these circumstances, and for want of correct notes, many of the experiments had to be repeated, and the whole to be again arranged. This, though a work of no great labour, yet with other circumstances connected, have hitherto prevented its appearance. It is expected, however, to be before the public about February next.

I am desirous to repeat some of your experiments with my multipliers, which have graduated circles proportioned to the needles. I omitted to mention that I place the needle, which acts as an index, above the coil, and its graduated circle likewise. This renders it easy to inspect the degrees. I also place another needle within the coil, properly situated. With this arrangement, a slight discharge causes a very active movement. It is not unlikely however, that this mode of experimenting may have been resorted to by you and others who have been much more engaged in electro-magnetic investigations than I have.

I am, Sir, with esteem,
Your obedient Servant,
ROBERT HARE.

Philadelphia,
April 5, 1832.

III. *On the Electro-chemical action exercised by simple metals on fluids. By* WILLIAM STURGEON, *Lecturer on Experimental Philosophy at the Honourable East India Military Academy, Addiscombe.*

In the year 1830 I published the first part of my "*Experimental Researches in Electro-magnetism, Galvanism, &c.*" in which are detailed several experiments bearing directly on the doctrine of the chemical action of metals on acid and other solutions; and in a note which commences at page 77, I have given a pretty exact statement of the peculiar views which I then took of the nature of certain chemical phenomena; and have illustrated those views by appropriate experiments. As it is possible that the work alluded to may not be known to many of the readers of this journal, it will be necessary that they be made acquainted with that part of it which relates immediately to the subject in question. It is as follows:

"Fig. 11 represents a glass jar, which is to be nearly filled with a limpid solution of sulphuric acid; in the jar is placed, in a sloping position, a slip of amalgamated rolled zinc; no gas will be evolved. Hold a slip of copper also in the solution, and no gas will appear while the two metals are separate. Let the lower edge of the copper slip touch the amalgamated piece, a cloud of gas will immediately ascend from every part of the copper surface, but not a single bubble from the amalgamated zinc. If the zinc be formed into a disc, and laid flat in the bottom of the jar, a copper wire held vertically, and touching the centre of the zinc, will liberate gas at every part of it which is below

the surface of the fluid; and ás no gas will rise from the zinc disc, this variation of the experiment will have a very pleasing effect.

This beautiful and interesting experiment appears to be exceedingly well calculated to direct our ideas to the very fountain of chemical action; and is, without exception, the most striking manifestation of the influence of simple galvanic arrangements in starting into active play those potent energies which are exercised in the chemical change of matter.

Whilst the metals are unconnected, the very powers which are capable of reducing their symmetrical forms into rude heterogeneous masses, are suspended within the boundaries of their own surfaces, or securely lodged in the fluid moleculæ which surround them. But if once the metals touch, the magic spell is broken—tranquility vanishes with the velocity of lightning,—and the once dormant powers of electricity, as if by enchantment, instantly spring into uncontrouled activity,—flow with immeasurable celerity, and precision of direction, through the solid and fluid group giving new *forms* and *positions* to their obedient elements. Again separate the metals, again the seal is closed,—the electric spring has lost its powers, and receded to within the barriers of the inactive elements, where it once more reposes in concealment.—Tranquility is thus restored, and chemistry ceases to exist.

If we would trace the analogy to more complex electro-chemical actions, some of the experiments already described, and others which will soon be spoken of, will offer considerable facilities to such an enquiry.

The electrical characters of metallic bodies become so very materially modified even by imperceptible transitions in the forms of their moleculæ, that it is next to impossible to select two specimens from the same mass, which are precisely in the same electrical state, and kindred pairs of some of the metals, such as iron, zinc, tin, &c. form very active galvanic combinations, the energies of which require no very nice galvanometer, nor experimental dexterity for their detection.

The difference, which by experiment is thus discovered in the electrical characters of considerable masses, is easily detected by a similar process in the smallest tangible fragments; and by carrying our ideas a little farther, we readily discover, that similar electrical relations may possibly exist in the very elementary metallic particles of which those fragments are composed. Hence it appears, that even the smallest atoms of the same metallic body may *relatively* be in different states of electricity; although the mass itself

taken as a whole, displays a decided and peculiar electric character in reference to all other metallic bodies.

With this view of the electrical state of metallic surfaces, it requires no superlative degree of penetration to discover that multitudes of galvanic circles must necessarily spring into active play, whenever a metallic body is plunged into a fluid medium suitable to the promotion of their energies; for, if we consider the two wires in the experiment (fig. 11) as a simple elementary pair of particles already in galvanic action, we can readily transfer our ideas to two particles in similar galvanic operation on the surface of a piece of zinc, iron, tin, &c. but as that surface, when extensive, consists of myriads of particles, all of which are relatively in different states of electricity, and in metallic contact with each other, there must also exist multitudes of elementary pairs in active operation; combining with oxygen, and liberating gas at their respective poles as decidedly as in any galvanic arrangement whatever; and their multitudinous character alone prevents our distinguishing them separately.

It will not be necessary, however, to extend our ideas so far as to consider that every ultimate particle absolutely constitutes a galvanic pole; the imagination will be considerably relieved, and it will also be more consistent with reason and observation, to contemplate those particles in congregated groups; the relative electrical characters of which alone will satisfy every enquiry as regards the decomposition of fluid menstrua by metallic bodies.

When a metallic body is plunged into an acid solution, the points on its surface, from which gas is liberated, are by no means so numerous as we are frequently led to imagine; and when the proportional quantity of acid to water is very small, those points may be easily distinguished from each other, and even enumerated by close attention.

Experiment a.—Let a glass tumbler be nearly filled with water, mixed with a few drops of sulphuric acid; introduce a piece of clean zinc, about the size of a half-crown, and let it rest at the bottom of the vessel. When this apparatus is perfectly steady, it will be observed, by looking attentively through the glass, that the points from which the hydrogen is liberated, are comparatively but very few in number. The experiment, when the acid is properly diluted, is exceedingly interesting, and has a very pleasing effect. The gas rises in distinct streams, which do not mix into a confused cloud till it has ascended to a considerable height above the metallic surface. These gaseous fountains may be regarded as so many galvanic poles, which will be more or less active, according to the relative

electric states of the different groups, or parts of the metallic surface; and also according to the relative electric states of the constituent parts of the fluid medium. The *number*, as well as the *energies* of those poles or fountains, will likewise be regulated by these electrical qualities of the solid and fluid parts.

It very often happens, that the fountains of gas are at some considerable distance from each other on the surface of the zinc plate; the observer may then select any one of them for a second experiment, which, in a philosophical point of view, is quite as interesting as the former, and proves most decidedly that those fountains are of galvanic origin; for their energies may be so completely controuled that the experimenter may exalt, diminish, or even annihilate them at pleasure, upon the most rigid galvanic principles.

Experiment b.—When an individual fountain has been selected, let a thin copper wire touch the zinc plate at a point not very distant from it; gas will not flow from the fountain so rapidly as at first, but will be copiously liberated at the surface of the copper wire. Bring the wire still nearer to the fountain, and the stream of gas will be much thinner, and it will become more and more slender as the wire approaches it; till at last, when the wire has arrived sufficiently near, the fountain will cease to play.—If the wire be made to recede gradually, the fountain will re-commence with the appearance of an exceedingly thin slender stream, the density and dimensions of which will increase as the wire becomes more distant; and when the wire is entirely removed from the zinc plate, the energies of the fountain will appear as active as at first.

Experiment c.—When several copper wires touch the zinc plate in different places, each wire will liberate gas, and the activity of the fountains on the surface of the zinc will be considerably abated. When the wires are sufficiently numerous, every one of the fountains will be extinguished, and gas will be liberated at the copper wires only; showing in a very impressive manner, that the *natural galvanic energies* of the zinc which produce those fountains are susceptible of being overpowered by the artificial *galvanic energies* which have superceded them, and which have produced new fountains of gas at the newly established *artificial poles*.

This variation of the experiment will give the observer a pretty good idea of what may be very appropriately termed *natural galvanic circles;* proving also that multitudes of them may exist in active play at the same moment, on the same metallic surface.

Experiment d.—The illustration will be considerably

improved, if the zinc plate be amalgamated, and a few coarse copper filings scattered on its surface. It will be observed that every particle of copper will become a galvanic pole, and liberate gas with the greatest promptitude, and highly imitative of the *natural galvanic process* on the surface of a piece of zinc when plunged into a similar solution.

Experiment e.—If, in place of the copper wire, (*ex. b.*) a narrow slip of amalgamated zinc be employed, the fountain, instead of being annihilated, will become more active by a close approximation of the amalgamated piece;—because the latter has an opposite electrical relation to the zinc, to that displayed by the copper, (article 45)* These appearances are very easily produced when the fountains are but few in number, and not very active; but it frequently happens when a slip of amalgamated zinc is employed, that part of the mercury runs to the other piece, and spreads itself over the fountain, which immediately becomes extinct, by having its electrical character changed.

To prevent confusion, it will be necessary to draw a line of demarcation between galvanic *combinations* and *circles* of the two different characters—*natural* and *artificial.*—*Natural galvanic combinations* are those which I consider to exist naturally in the same piece of metal; whether it be an *amalgum*, an *alloy*, or a *pure metal;* the latter class of which, I imagine, are but very few in number. These *natural combinations* are brought into play when the piece is plunged into a fluid medium suitably adapted to promote their energies; as for example, zinc into diluted sulphuric acid. When thus situated, *natural galvanic circles* are formed, the energies of which I consider to be the cause of what is termed the *chemical action* between the metal and the acid solution; and consequently *that action*, a *galvanic phenomenon.*

Artificial galvanic combinations are all those which are formed of *two* or *more* pieces, whether those pieces be of *two* or more distinct metals; or of the *same kind* of metal in different states of compression, figure, polish, size, &c., or of any combination whatever consisting of more than *one piece of metal*, and *one fluid medium.* Hence the copper wires, or filings, with the zinc plate in the preceding experiments, formed *artificial galvanic combinations;* and were merely illustrative of the *natural galvanic combinations* which are considered to exist only in the self-same piece.

Now, it is evident by the illustrative experiment *d*, that all the natural combinations on the surface of a piece of metal may be contemplated collectively, and regarded as one

* Of the Original.

combination only; for the amalgamated piece of zinc, giving off no gas, can have no very energetic natural combinations; and those which do exist are too insignificant to compete with the artificial combination formed by the plate and one single particle of copper only. These natural combinations may, therefore, be considered as absolutely *neutral* amongst themselves, when several particles of copper are in contact with the amalgamated plate, the surface of which may then be regarded as in *one uniform state of electricity*; and the particles of copper as uniformly in the opposite state. But each particle of copper liberates gas; and is consequently a galvanic pole, and necessarily belongs to a galvanic circle, which possesses a pole of the opposite character. This latter pole is to be found in the amalgamated zinc; and to the *same source* we are to trace as many galvanic circles as there are particles of copper in contact with it. But the amalgamated zinc is in *one uniform state of electricity*, and therefore notwithstanding the *number* and *individuality* of poles which it supplies, this uniformity of its electrical character pronounces it to be *one individual aggregate*, or concentrated pole, to all the existing galvanic circles. Precisely the same reasoning holds good by reversing the experiment; for if several particles of zinc be placed on a copper plate, each particle will become a galvanic pole; and the copper, by embracing all the opposite poles in one *uniform electric character*, will become *one aggregate* or concentrated pole to as many galvanic circles as there are particles of zinc in contact with it. Hence we discover that a galvanic combination may be divided into as many branches as we please. It may have one *positive* pole and one *negative* pole; it may have one of either character, and as many of the other as we please; or it may have a multiplicity of both at the same time, and still be *one* galvanic combination only. It appears, therefore, that much of the difficulty in comprehending the multitudinous character of natural galvanic combinations speedily vanishes by this mode of reasoning; for whether we contemplate those combinations *separately* or *collectively*,—whether as an *assemblage* of combinations, or as *one* combination only, we uniformly arrive at the same conclusion, viz., that a number of *points* on the surface of a piece of metal, although *relatively* in different states of electricity, are, when taken *collectively*, and as a whole, uniformly in the *same state*, as regards all the rest of the surface, which must of necessity, be as uniformly in the *opposite* electric state. Those points will, therefore, become distinct galvanic poles to circles, which may be traced to one *aggregate* or *concentrated* pole of the opposite kind.

Now, as those points or poles, which have hitherto been considered separately, and distinct from each other, are all of the same electric character (say negative) as regards the aggregate positive pole; *they* also may be regarded as forming one aggregate *negative pole*, and consequently, the whole of the metallic surface in *two distinct states* of electricity. By this simplification, we find, that notwithstanding the multitudes of natural galvanic combinations which may possibly exist on the surface of a piece of metal; and the various changes of energy and position, which they may experience during the natural galvanic process, they will always be divided into two *grand divisions*, which will be *positive* and *negative* as regards each other; and the metallic surface itself will, at all times, operate as *one natural galvanic combination only*.

The reader will now find little difficulty in comprehending what I have considered as *natural galvanic combinations*. They are supposed to exist in all the metals, and perhaps in other bodies, but with different degrees of energy in each. These views of *natural galvanic combinations* appear to me well calculated to remove some of those difficulties which have hitherto been stumbling blocks in this branch of philosophy; but with what reception they may experience from others, is not for me to determine.

Considering that *heat* might possibly develope the energies of natural metallic combinations, I was induced to make experiments with single pieces of metal, and have found that some of them become highly *electro-magnetic* by that process. The experiments will be described in subsequent pages."

Whatever may be the cause which gives a different electric character to dissimilar metals, it is reasonable to expect that the same influence will be extended to the particles of other dissimilar matter, and that the simple gases have each a peculiar electric character, and that when combined, either in simple pairs, or in any other proportions, the compound particles will necessarily be as distinctly electro-polar as the copper and zinc in a voltaic pair;* and as susceptible of polar separation, by superior

* The Voltaic plates, which are usually manufactured in London and sold in the Instrument makers' shops, are a pair of discs, one of copper, the other zinc; which vary in size from four to ten or more inches in diameter, according to the fancy of customers. One surface of each disc is made perfectly flat and smooth; the other side of each is furnished with a socket, in which is cemented a glass handle, for the purpose of insulating the plates from the hands of

electric forces, as the particles of red lead and sulphur in the long neglected but highly interesting experiments of Lichtenburg and Cavallo.* Hence it is, and from no other known cause, that chemical compounds are decomposable and their constituent elements uniformly arranged by electric action: some requiring a greater, and some a lesser degree of force to effect their separation.

The electro-polar forces naturally existing in some of the metals, even in the purest state they have hitherto been procured, are, perhaps, more energetic than those of any single voltaic pair that can be formed of two distinct pieces of dissimilar metals; and some of the most formidable of those forces, (on the surfaces of metals in common use,) are presented by iron and by zinc.

If a piece of zinc or iron be immersed in distilled water, its polar particles and those of the water being brought to within the sphere of each other's action, begin to operate on each other; and the reciprocal polar forces mutually assisting each other, are enabled to separate the constituent particles of the aqueous compound: the oxygen being carried to the *positive* poles of the metallic surface, and the hydrogen to the *negative* poles, according to the well-known laws of electro-chemical action by the energies of the voltaic pile. By this means a continual interchange of the electric fluid takes place between the polar points on the surface of the metal, and the fluid particles to which they are presented; and multitudes of elementary electric

the operator. When these plates are held by the glass handles, and their smooth faces brought into close contact, a portion of the electric fluid which previously occupied the copper, passes to the zinc plate, which is easily detected by separating the plates suddenly, and applying either of them to the cap of a gold leaf electroscope (an instrument to be described in a future number of these Annals.) The zinc exhibits a state of *positive*, the copper that of *negative* electrization. Instead, however, of insulating both plates, it will be found more advantageous to insulate that only whose electricity, after contact, is to be exhibited.

* If the powders of red lead and sulphur be intimately mixed together, and put into a spring puff, such as hair-dressers sometimes use for powdering wigs, and then projected from the puff through the air, against a resinous cake or paper tea-board, the different parts of whose surface is in different electric conditions, or, if you please, some parts positively and others negatively electric; the particles of the sulphur will fly to the positive surfaces, and those of the red lead to the negative surfaces; showing that the red lead and sulphur were in different electric conditions. Many other mixtures of powders show a similar phenomenon.

currents are thus produced, which flow through the metallic and fluid media in *directions* as exact as any voltaic pair; and those directions as diversified as the variety of positions of the elementary poles in their general distribution over the surface of the metal.

The positions of the polar points on the surface will frequently change during the dissolution of the metal on various accounts; depending upon the deposition of oxide, formed by the first and subsequent currents, the texture of the metallic points, their polish as they are arrived at during the process, their crystaline structure, and perhaps upon many other causes of a still more recondite nature. Every succeeding surface of the metal will thus present its peculiar arrangement of assailing poles to the contiguous fluid particles, which, in consequence of their electric omniparity,—susceptibility and independency of motion, offer neither inequality of force, nor united energy to resist the attack; but obsequiously arranging themselves under the influence of their assailants, become vanquished *individually* at every point; and resign their constituent elements to the superior electric forces to which they are continually exposed.

It is true that the decomposition of distilled water is slow by the natural polar energies on the surface of a piece of zinc, or of a piece of iron, but still the process is considerably more rapid than that which would proceed from the electric action of any binary metallic combination, on whose surfaces, whilst separate, the natural electro-polar forces, are not energetically displayed. But if the polar energies be of an exalted character on zinc and on iron, they are infinitely more so on the surfaces of sodium and potassium, metals which decompose water with an amazing rapidity.

It will now be easily understood how it happens, that a piece of zinc, (and the same reasoning applies to iron and some other metals) which, before it entered the water, was quite smooth and clean, becomes rough and asperous by long immersion; for the oxygen being the only part of the water which unites with the metal, becomes unequably distributed over its surface, at its positive poles only: the negative poles of the metallic surface having nothing to combine with, are left unmolested, and for a while remain as prominent as at first But as the process advances, the changes of position of the metallic poles, which are continually going on, being attended by corresponding changes, in their electric conditions, those points on the surface, which previously had been electro-negative, will occasionally become electro-positive, and consequently will, in *their* turn, become blended with oxygen, liberated from the

aqueous particles to which they are immediately presented: hence it is, that every part of the surface becomes eventually oxidized. But at whatever period of the process we examine the metal, its surface is found to be asperous; which is a natural consequence of the multitude of electric poles, which had been in play in the electro-chemical process.

I have selected distilled water for the fluid medium, merely to simplify the illustration of the view which I have taken of the cause of this class of chemical phenomena; but the same principles equally apply to the dissolution of metals in other fluid menstrua, and are much more beautifully displayed when the action is of a more vigorous character. If, for instance, the water were to be mixed with an acid, (say the sulphuric), the polar energies would become considerably more active, and the decomposition of the liquid, and disappearance of the metal, proportionally more rapid. The surface of the latter would soon become exceedingly rough, but the asperities would frequently change their positions before the metal entirely disappeared.

Moreover, a rough zinc surface promotes this voltaic action to a greater extent than one which is smooth.—Hence it is, that the action is not so formidable whilst the zinc surface is smooth and clean, at the first immersion, as it is a short time afterwards, when the surface has assumed a more decided asperous character. And hence also, that cast zinc has more formidable poles than rolled zinc; not only at first, but during the whole time of the metal's dissolution; and consequently becomes more rapidly destroyed in similar acid solutions.

It is also on this account that *cast* zinc loses a greater portion of its real electric character, as a distinct metal, in the voltaic series, than rolled zinc does; and forms with copper, a feebler combination, than when in the latter compressed state;* and when the natural electric poles, on a tolerably smooth surface, become nearly extinct by a covering of mercury, the zinc assumes its real electric character more decidedly, than in either of its former con-

* Should Dr. Faraday happen to read this paper, as in all probability he will, it may possibly remind him of the *first* source of his information respecting the superiority of *rolled* over *cast* zinc in the construction of Voltaic batteries; a piece of information which he has neglected to communicate to the readers of the Philosophical Transactions for 1835. Dr. Faraday's polite concessions, however, in other similar cases, leave no doubt of his early attention to this.

ditions. On which account, it forms with copper a still more formidable voltaic combination, than even pure zinc rolled does, without such electro-neutralizing amalgamation.

Experiment.—If a slip of zinc, of any breadth whatever, bent into the form *a, o, b,* fig. 12, and have its extremities *a* and *b,* immersed to the same depth in an acid solution; a compass needle, *n s,* placed as in the figure, will soon indicate the presence of an electric current in the parts between which it is placed: and by its various movements, and occasional stationary positions, we learn that electric currents set in, sometimes in one direction and sometimes in the other, during the dissolution of the metal in the fluid menstrua; which is a consequence of the manifold changes of number, position, and energy of the natural electric poles on the surface of each immersed extremity of the metal.

Experiment.—If the end *a,* of another similarly formed slip of zinc, be immersed in a strong nitric acid solution, and the end *b,* in a feeble one, placed in two cells of a trough or box, having a bladder partition between them; the needle will soon be steadily deflected, indicating a current to be running along the metal *from a* to *b,* a phenomenon easily accounted for upon the principles I have been endeavouring to establish: for the stronger acid solution giving greater facility to the play of the electric currents proceeding from the polar energies on the surface of the metal immersed in it, than the feeble acid solution does to the play of electric currents on the other surface; much of the real electric character of the extremity *a* will be lost, or, if you please, it will receive *more* fluid from the stronger acid solution than is received by the other extremity from the feebler. On which account it will be in precisely the same electric relation with the other extremity as copper is with zinc, and will consequently operate as a distinct metal in combination with the extremity *b,* which has not its natural electric character so much disturbed.—A slip of copper exhibits facts of a similar nature.

I know of no received theory of chemical action that will satisfactorily explain the *corrosion* of metallic surfaces when exposed to acid and other fluids: nor why some metals are dissoluble by almost any liquid to which they are exposed, whilst others require certain acid or alkaline fluids to accomplish their complete dissolution. These problems which have so long been shrouded in mystery may now probably receive a natural and easy solution.—The surfaces of the purest metals are unequably electrical, and consequently electro-polar: but their natural combinations are less powerful than those on the surface of metals

of a coarser texture. Hence it is, that acid particles susceptible of separation by feeble electric forces, become necessary to bring their polar arrangements into electro-dynamic play. This once accomplished, their disfiguration is certain, and the multitudes of electric whirlpools to which they are exposed, eventually terminate their solid existence.

The same principles which explain the dissolution of metals in acid and other solutions, apply equally to the precipitation of metals from those liquids of which they form a part. A piece of iron immersed in a solution of sulphate of copper will presently have its natural electro-negative poles capped with metallic copper; which are so exceedingly numerous that the surface becomes nearly cased in a very short time. The positive poles, in the mean time, are supplied with oxygen from the liquid, which unites with the iron, and a new compound is formed. The copper, now attached to the iron, and the uncovered parts of the latter metal, form new voltaic combinations, and the deposition goes on whilst there is a sufficient degree of electric energy to withdraw the cuperous particles from the liquid in which they are suspended.

The lead tree,* the arbor Dianæ, and other metallic arborizations, are phenomena of the same class, and are

* The *Lead tree* or *Philosophical tree*, as it is sometimes called, is thus made. Dissolve an ounce of acetate of lead in about half a pint of water. When the sediment has settled, pour the clear liquor into a large vial; and suspend in it a piece of clean zinc.—The bottle is now to be placed where it will not be shaken, and in a few hours brilliant films or metallic leaves will be suspended from various parts of the zinc surface.

When a large metallic arborization is wanted, a plain globular decanter will give it the best appearance. The solution, in proper proportions, may nearly fill the decanter; and a stout piece of either rolled or cast zinc suspended in the axis of the vessel, as before stated.

If a piece of zinc and another of copper be united end to end by solder, and bent into the form of the letter *u*, and suspended by the bend in a solution of acetate of lead; these two metals and the liquid form an artificial voltaic circle: and lead will be re-metalized in beautiful crystal on the surface of the copper; but not in such abundance as at the natural negative poles on the surface of the zinc.

The *Arbor Dianæ*. "If a little mercury be poured into a bottle nearly filled with solution of nitrate of silver, and the bottle be left for some time undisturbed, the silver is precipitated in a beautiful form, resembling the branches of a tree, which is the *Arbor Dianæ*." *Henry's Chemistry*.

susceptible of a similar explanation; so are the amalgamations of metallic surfaces by the interference of acid and other solutions. Heat very often facilitates the process, and is sometimes indispensable to liquify the covering metal. Such is the case in the various processes of tinning iron and other metals, either in sheets by dipping or otherwise, and in the process of soldering. The implement called a soldering iron, when soft solder is used, is absolutely a lump of copper. When this is heated just to below redness, it becomes coated with solder much more readily by the intervention of muriatic acid than by anything else. And iron or zinc, to which the tinmen usually apply a solution of muriate of ammonia, is tinned with much more facility by covering the surface with dilute muriatic acid.

The different degrees of facility with which metals and oxygen combine with each other, is a problem in chemical science which by no means finds an intelligible solution in the doctrines usually applied. The production of the lead tree, already mentioned, may again be referred to as illustrative of this fact; and of the imperfection of every theory of the formation of that beautiful product which regards not the electro-polarity of the zinc, and the influence of electric currents as the most essential of its operative principles. The latter of these principles has long ago been admitted to perform an active part in every period of the process subsequently to the first attachment of the re-metalized lead to the surface of the zinc. But where are we to look for a satisfactory explanation of the cause which liberates and leaves suspended the *first* particles of the lead? The "superior affinity of oxygen for zinc" has long been a subterfuge, rather than a satisfactory solution of this curious problem. The doctrine of affinity would at once shroud the zinc surface with oxygen to the entire exclusion of the revived lead, which, having no access, could have no opportunity to cling to the metallic zinc; but would fall, by its superior gravity, to the lower parts of the fluid. If it be admitted that the first revived particles would, for a time, remain suspended, still the subsequent part of the process would remain enveloped in obscurity, because of the want of metallic contact between the zinc and the liberated lead; and to admit a sufficiency of conductability in the oxide, would be an unnecessary and unscientific concession, whilst more familiar principles are at our command, and an easy and natural explanation placed conspicuously before our eyes.

(To be resumed.)

IV. *British Association for the Promotion of Science.* SIXTH MEETING, BRISTOL—MR. HERAPATH, on *Arsenical Poisons.*

Mr. Herapath observed, that as arsenical poisons were obtained with so much facility, and that their operation was so deadly, that they were the principle means resorted to by secret poisoners. It became, therefore, essential to the community, that every new fact relating to their administration, operation or detection, should be made known. He was not aware that any well-authenticated case had been published, in which death had been occasioned by realgar or red arsenic; but the Burdock case was one of this kind. It would, perhaps, be remembered that the victim, Mrs. Smith, had been buried fourteen months; that upon exhumation, orpiment was found in the stomach, and the body was partly converted into adipocere. In prosecuting his experiments in this case, he conceived the idea of identifying the poison found, with that sold to the witness Evans, by Hobbs, the druggist; through an impurity he discovered in the poison of the stomach. With this view, he purchased some out of the same box, and requested that it might be of the same kind as that sold to the prisoner's agent. It then transpired that the box contained three different substances mixed together; white, yellow and red arsenic; the two former in small lumps, the latter in powder; that it was the powder of realgar only, which had been administered, although it was undoubtedly found as yellow orpiment in the exhumed body. In tracing the possibility of change, he found that two agents, sulphurated hydrogen and ammonia, would either of them convert realgar into orpiment. Now, as it was well known that both of these gases were evolved during putrid decomposition, there could be no difficulty in accounting for the change of colour; but, to place the matter beyond a doubt, he made a direct experiment by poisoning an animal with some of the same kind of realgar, and found that, after putrefaction it had been changed as in the case of Mrs. Smith. It would, perhaps, be recollected, that the conviction of the prisoner, was mainly owing to the evidence of a little girl, who deposed that she saw Mrs. Burdock put a powder into some gruel, and afterwards to administer it to Mrs. Smith. At that time considerable doubt was entertained of the truth of her evidence, from its being invariably precise to a word; and also from the difficulty of believing that any person would be found so fool-hardy as to mix and administer poison before a child or stranger. But what she had

stated proved to demonstration that her evidence was correct, for she said that the gruel given "was of a nasty *red* colour;" a colour she could not have had an idea of unless she had seen it, as nothing had transpired of red arsenic; and had she invented a tale to account for the appearance of the body, or had she spoken from what she heard from others, she would have deposd to its being of a yellow colour.

From what had occurred, therefore, it was clear that the realgar of the shops would cause death; that half an ounce given at twice (by the prisoner's confession) was sufficient for that purpose; that realgar became orpiment during putrefaction; that realgar, like arsenous acid, had a tendency to control putrefaction and convert bodies into adipocere. During the experiments upon this case, he found that the microscope system of testing, which was first introduced by Dr. Wollaston, and which he had himself constantly followed, could be made to improve the very beautiful reducing process proposed by Dr. Christison, and also furnished an excellent method of proving to the jury the presence of arsenic. Mr. Herapath here described several chemical tests by which the presence of arsenic may be discovered, and also described the method by which he found arsenic in the case of Sophia Edney, who was convicted at the March assizes at Taunton, of poisoning her husband; and concluded by observing that the recent plan of discovering arsenous acid by converting it into arsenuretted hydrogen, and depositing the arsenical crust during its combustion, was the most elegant that could be conceived;* at the same time that it was the most sensitive: but it would require a few modifications to make it the best for exhibition to a jury. First, it was essential that the zinc used to procure hydrogen should have been treated by the experimenter in the same way without arsenic, otherwise the counsel would embarrass the witness by asking if he was certain that arsenic was not contained in the zinc;† and next, the metallic crust should be so received as to be kept from atmospheric air, otherwise it would lose its lustre by passing into the "fly powder" of the Germans. Mr. Herapath had found it best to proceed thus:—instead of a plate of glass to cool the flame and receive the crust, he used one of mica, with three drops of water in separate

* Mr. Herapath not having mentioned to whom this beautiful process is due, we conceive it our duty to acquaint the reader that the discovery belongs to our ingenious fellow labourer, Mr. Marsh. See the next article.—*Edit.*

† Mr. Marsh has amply explained that precaution.—See page 32—Edit.

places on one of its surfaces; if the flame was allowed to play under one of these drops, the evaporation of the water kept the part cool, and the crust was thicker, while the risk of fracture was avoided: then, by inverting the plate, and holding the drops in succession some little height over the flame, they became solutions of arsenous acid and could be tested with three reagents as before stated; and, if it was necessary to make a quantitative experiment, the products of the flame could be condensed in a large globe; the arsenous acid dissolved and precipitated by sulphurated hydrogen. The part of the plate of mica containing the crust should then be cut off, and introduced into glass tubes hermetically sealed, like the slips of blotting paper, containing the coloured results of the reagents.

V. SEPARATION OF ARSENIC. *From the Transactions of the Society of Arts.* Vol. li.*—*The* LARGE GOLD MEDAL *was presented to* MR. JAMES MARSH, *of the Royal Arsenal, Woolwich, for his Method of separating Small Quantities of Arsenic from Substances with which it may have been mixed.*

NOTWITHSTANDING the improved methods that have of late been invented for detecting the presence of small quantities of arsenic in the food, in the contents of the stomach, and mixed with various other animal and vegetable matters, a process was still wanting for separating it expeditiously and commodiously, and presenting it in a pure unequivocal form for examination by the appropriate tests. Such a process should be capable of detecting arsenic not only in its usual state of white arsenic or arsenious acid, but likewise in that of arsénic acid and of all the compound salts formed by the union of either of these acids with alkaline substances. It ought, also, to exhibit the arsenic in its reguline or metallic state, free from the ambiguity which is sometimes caused by the use of the carbonaceous reducing fluxes. It appeared to me, that these objects might be attained by presenting to the arsenic hydrogen gas in its nascent state: the first action of which would be to deoxygenate the arsenic; and the next, to combine with the arsenic, thus deoxygenated, into the well known gas called arsenuretted hydrogen. Being thus brought to the gaseous state, the arsenic would spontaneously (so to speak) separate itself from the liquor in which it was before dissolved, and might be collected for examination by means of any common gas apparatus; thus

* By permission of the Author.

avoiding the trouble, difficulty, and ambiguity of clarification and other processes whereby liquors, suspected of containing arsenic, are prepared for the exhibition of the usual tests, or of evaporation and deflagration which are sometimes had recourse to in order to separate the arsenic from the organic substances with which it may have been mixed.

I had the satisfaction of finding, on trial, that my anticipations were realized; and that I was thus able, not only to separate very minute quantities of arsenic from gruel, soup, porter, coffee, and other alimentary liquors, but that, by continuing the process a sufficient length of time, I could eliminate the whole of the arsenic in a state of arsenurated hydrogen, either pure or at most only mixed with an excess of hydrogen.

If this gas be set fire to as it issues from the end of a jet of fine bore into the common air, the hydrogen, as the more combustible ingredient, will burn first, and will produce aqueous vapour, while the arsenic will be deposited either in the metallic state, or in that of arsenious acid, according as it is exposed partially or freely to the air. The former condition is brought about by holding a piece of cold window glass opposite to, and in contact with the flame, when a thin metallic film will be immediately deposited on its surface; and the latter, by receiving the flame within a glass tube open at both ends, which in half a minute, will be found to be dimmed by a white pulverulent sublimate of arsenious acid. By directing the flame obliquely within side of the tube, it strikes against the glass, and deposits the arsenic partly in a metallic state. In this case, if the tube, while still warm, be held to the nose, that peculiar odour, somewhat resembling garlic, which is one of the characteristic tests of arsenic, will be perceived. Arsenuretted hydrogen itself has precisely the same odour, but considerable caution should be used in smelling to it, as every cubic inch contains about a quarter of a grain of arsenic.

The requisite apparatus is as simple as possible; being a glass tube open at both ends, and about three quarters of an inch in its internal diameter. It is bent into the form of a syphon (*aa*, fig. 13) the shorter leg being about five inches, and the longer about eight inches in length. A stop cock, *b*, ending in a jet of fine bore, passes tightly through a hole made in the axis of a soft and sound cork, which fits air-tight into the opening of the lower bend of the tube, and may be further secured, if requisite, by a little common turpentine lute. To fix the apparatus, when in use, in an upright position, a hole is made in the wooden block *c* for the reception of the lower part of the pillar *d*, and a groove

is cut in the top of the same block to receive the bend of the tube *a a*. Two elastic slips *e e*, cut from the neck of a common bottle of India rubber, keep the tube firm in its place.

The matter to be submitted to examination, and supposed to contain arsenic, if not in a fluid state, such as pastry, pudding, or bread, &c., must be boiled with two or three fluid ounces of clean water, for a sufficient length of time.

The mixture so obtained must then be thrown on a filter to separate the more solid parts: thick soup, or the contents of the stomach, may be diluted with water, and also filtered; but water-gruel, wine, spirits, or any kind of malt liquor and such like, or tea, coffee, cocoa, &c. can be operated on without any previous process.

When the apparatus is to be used, a bit of glass rod, about an inch long, is to be dropped into the shorter leg, and this is to be followed by a piece of clean sheet zinc, about an inch and a half long and half an inch wide, bent double, so that it will run down the tube till it is stopped by the piece of glass rod first put in. The stopcock and jet are now to be inserted, and the handle is to be turned so as to leave the cock open. The fluid to be examined, having been previously mixed with from a drachm and a half to three drachms of dilute sulphuric acid (1 acid and 7 water) is to be poured into the long leg, till it stands in the short one about a quarter of an inch below the bottom of the cork. Bubbles of gas will soon be seen to rise from the zinc, which are pure hydrogen if no arsenic be present, but if the liquor holds arsenic in any form in solution, the gas will be arsenuretted hydrogen. The first portions are to be allowed to escape, in order that they may carry with them the small quantity of common air left in the apparatus; after which the cork is to be closed, and the gas will be found to accumulate in the shorter leg, driving the fluid up the longer one, till the liquor has descended in the short leg below the piece of zinc, when all further production of gas will cease. There is thus obtained a portion of gas subject to the pressure of a column of fluid of from seven to eight inches high; when, therefore, the stopcock is opened, the gas will be propelled with some force through the jet, and, on igniting it as it issues (which must be done quickly by an assistant,) and then holding horizontally a piece of crown or window glass (*f*, fig. 13) over it, in such a manner as to retard slightly the combustion, the arsenic, (if any be present) will be found deposited in the metallic state on the glass; the oxygen of the atmosphere being employed in oxydizing the hydrogen only during the process. If no arsenic be present, then the jet of the flame as

it issues has a very different appearance; and, although the glass becomes dulled in the first instance by the deposition of the newly formed water, yet such is the heat produced, that in a few seconds it becomes perfectly clear, and frequently flies to pieces.

If the object be to obtain the arsenic in the form of arsenious acid, or white arsenic, then a glass tube from a quarter to half an inch in diameter (or according to the size of the jet of flame) and eight or ten inches in length, is to be held vertically over the burning jet of gas, in such a manner that the gas may undergo perfect combustion, and that the arsenic combined with it may become sufficiently oxydized; the tube will thus, with proper care, become lined with arsenious acid in proportion to the quantity originally contained in the mixture.

When the glass tube is held at an angle of about forty-five degrees over the jet of flame, three very good indications of the presence of arsenic may be obtained at one operation; viz. metallic arsenic will be found deposited in the tube at the part nearest where the flame impinges,—white arsenic or arsenious acid at a short distance from it, and the garlic smell can be readily detected at either end of the tube in which the experiment has been made.

As the gas produced during the operation is consumed, the acid mixture falls into the short limb of the tube, and is thus again brought into contact with the the zinc, in consequence of which a fresh supply is soon obtained.—This gas, if submitted to either of the processes before described, will give fresh indications of the presence of the arsenic which the mixture may have originally contained; and it will be easily perceived that the process may be repeated as often as may be required, at the will of the operator, till no further proofs can be obtained.

When certain mixed or compound liquors are operated on in this apparatus, a great quantity of froth is thrown up into the tube, which may cause a tittle embarrassment by choking the jet. I have found this effect to take place most with the contents of the stomach, with wine, porter, tea, coffee or soup, and, indeed, with all mucilaginous and albuminous mixtures. The means I adopt to prevent this effect from taking place, or, at least, for checking it in a great measure, is to grease or oil the interior of the short limb of the apparatus before introducing the substance to be examined, or to put a few drops of alcohol or sweet-oil on its surface previously to introducing the stopcock and its appendages. I have, however, found, if the tube be ever so full of froth in the first instance, that, in an hour or two, if left to itself, the bubbles burst, and the interior of

the tube becomes clear without at all affecting the results.

In cases where only a small quantity of the matter to be examined can be obtained, I have found a great convenience in using the small glass bucket (*g*, fig. 14). Under such circumstances, the bent glass tube may be filled up to within an inch of the short end with common water, so as to allow room for the glass bucket, which must be attached to the cork, &c. by means of a little platina wire; a bit or two of zinc is to be dropped into the bucket, with a small portion of the matter to be examined, and three or four drops of diluted sulphuric acid (acid 2, water 14); and the whole is then to be introduced into the mouth of the short limb of the tube. The production of gas under this arrangement is much slower, and, of course, requires more time to fill the tube, than in the former case; but the mode of operating is precisely the same. Indeed it is of great advantage when the quantity of arsenic present is very minute, not to allow the hydrogen to be evolved too quickly, in order to give it time to take up the arsenic.

A slender glass funnel will be found of service when as much as a table-spoonful, or even a tea-spoonful of matter can be obtained for examination. In this case the tube is to be partly filled with common water, leaving a sufficient space for the substance to be examined; a piece of zinc is to be suspended from the cork by a thread or wire, so as to hang in the axis of the tube; and the fluid to be operated on, having previously been mixed with dilute sulphuric acid, is then to be poured through the funnel carefully, so as to surround the zinc, avoiding, as far as possible, to mix it with the water below, and the stopcock and its appendages are to be replaced in the mouth of the tube; the production of the gas then goes on as before stated, and the mode of manipulating with it is exactly the same as described in the foregoing part of this paper.

It will be necessary for me, in this place, to explain the methods I employ after each operation, to determine the integrity of the instrument, so as to satisfy myself that no arsenic remains adhering to the inside of the tube, or to the cork and its appendages, before I employ it for another operation.

After washing the apparatus with clean water, a piece of zinc may be dropped in, and the tube filled to within half an inch of the top of the short limb; two drachms of diluted sulphuric acid are then poured in, and the stopcock and cork secured in its place; hydrogen gas will in this case, as before, be liberated, and fill the tube. If the gas as it issues from the jet be then inflamed, and a piece of window glass held over it as before described, and any

arsenic remains, it will be rendered evident by being deposited on the glass; if so, this operation must be repeated till the glass remains perfectly clean, after having been exposed to the action of the gas.

When I have had an opportunity of working with so large a quantity of mixture as from two to four pints (imperial measure,) I then have employed the instrument, (fig. 15) which is indeed, but a slight modification of one of the instantaneous light apparatusses now so well known and used for obtaining fire by the aid of a stream of hydrogen gas thrown on spongy platinum. It will, therefore, be of importance only for me to describe the alteration which I make when I employ it for the purpose of detecting arsenic. In the first place, I must observe, that the outer vessel *a*, which I use holds full four pints, and that the jet of the stopcock is vertical, and its orifice is twice or three times larger than in the instrument as generally made for sale, and also that there is a thread of wire attached to the cork of the stopcock *b*, for suspending a piece of zinc *c*, within the bell-glass.

With an instrument of this description, I have operated on one grain of arsenic in twenty-eight thousand grains of water (or four imperial pints) and have obtained, therefrom upwards of one hundred distinct metallic arsenical crusts.

Similar results have been obtained with perfect success from three pints of very thick soup, the same quantity of port wine, porter, gruel, tea, coffee, &c. &c.

It must, however, be understood, that the process was allowed to proceed but slowly, and that it required several days before the mixture used ceased to give indication of the presence of arsenic, and also, a much larger portion of zinc and sulphuric acid was employed from time to time, than when working with the small bent tube apparatus, in consequence of the large quantity of matter operated on under this arrangement.

With the small apparatus, I have obtained distinct metallic crusts, when operating on so small a quantity as one drop of Fowler's solution of arsenic, which only contains one-120th part of a grain.

The presence of arsenic in artificial orpiment and realgar, in Scheele's green, and in the sulphuric of antimony, may be readily shown by this process, when not more than half a grain of any of those compounds is employed.

In conclusion, I beg to remark, that although the instruments I have now finished describing, are the forms I prefer to all that I have employed, yet it must be perfectly evident to any one, that many very simple arrangements might be contrived. Indeed, I may say unequivo-

cally, that there is no town or village in which sulphuric acid and zinc can be obtained, but every house would furnish to the ingenious experimentalist ample means for his purpose; for, a two-ounce phial, with a cork and piece of tobacco-pipe, or a bladder with the same arrangement fixed to its mouth, might in cases of extreme necessity, be employed with success, as I have repeatedly done for this purpose.

The only ambiguity that can possibly arise in the mode of operating above described, arises from the circumstance that some samples of zinc of commerce themselves contain arsenic; and such, when acted on by dilute sulphuric acid, give out arsenuretted hydrogen. It is, therefore, necessary for the operator to be certain of the purity of the zinc which he employs, and this is easily done by putting a bit of it into the apparatus with only some dilute sulphuric acid; the gas thus obtained is to be set fire to as it issues from the jet; and if no metallic film be deposited on the bit of flat glass, and no white sublimate within the open tube, the zinc may be regarded as in a fit state for use.

JAMES MARSH.

VI. *An Examination of the Question, whether the Discordancy between the Characteristics of Mechanical Electricity, and the Galvanic or Voltaic Fluid, can arise from Difference of Intensity and Quantity; with some Observations in Favour of the Existence of an Electromotive Power independently of Chemical Reaction, but co-operating therewith:—respectfully submitted to the British Association for the advancement of Science. By* R. Hare, *M.D., Professor of Chemistry in the University of Pensylvania.**

In one of the papers, giving an account of Faraday's recent valuable researches in electricity, for copies of which I have been indebted to the flattering attention of the author, I find the following language:

"Hence arises still further confirmation, if any were required, of the identity of common and voltaic electricity, and that *the differences of intensity and quantity are quite sufficient to account for what were supposed to be their distinctive qualities.*" And elsewhere referring to Cavendish, as the author of this opinion, it is alleged that it "*only requires to be understood in order to be admitted.*"

* From a pamphlet by the author.

Notwithstanding that in support of the opinion thus quoted, the much respected authority of both Cavendish and Faraday is arrayed, it is one which I cannot so understand as toadmit.

I am unable to form any other idea of intensity, than that of the ratio of quantity to space. Thus the intensity of the pressure of an elastic fluid, is as the quantity to the space in which it is confined. The space being the same, the intensity of the pressure will be directly as the quantity; the quantity being constant, the pressure will be inversely as the space.

Agreeably to an analagous mode of reasoning, the intensity of the light or the heat emanating from a radiant body, is always estimated to be inversely as the surfaces on which it may be diffused or concentrated : and hence the inference that the intensity is as the square of the distance inversely, or as the area of the receiving surface of a lens or mirror, to that of the focus into which the rays are collected.

It follows that if there be in any two cases, like quantities of electricity evolved by mechanical, and by galvanic apparatus, the space occupied by the fluid generated by the latter, must be much greater, as its intensity is less.

In a memoir which I published upon this subject some years ago, I endeavoured to show that the spaces occupied by equivalent charges of galvanic and mechanical electricity were not such as to justify the idea that the former required for its existence a larger space than the latter. But on this subject, it is not now necessary to recur to the facts which I then adduced, since I find it conceded in one of the recent memoirs of Faraday, that the spaces occupied by the electricity evolved by galvanic apparatus, as compared with those occupied by mechanical electricity, are almost infinitely small. "*A grain of water or of zinc, contains as much of the electric fluid as would supply eight hundred thousand charges of a battery containing a coated surface of fifteen hundred square inches.*" "*Four grains of zinc, with one of water, may yield as much electricity as is evolved during a thunder-storm.*"

It follows inevitably that the electric matter evolved by galvanic action, is previous to its evolution, in a state almost infinitely intense, as compared with that of the same matter when evolved by a machine or meteorological changes. Yet to the currents induced in this matter, in the first mentioned form, an opposite state is ascribed, as respects intensity, to that in which it has previously existed.

It may be said with respect to currents, that the space being the same, the intensity will not only be directly as the quantity but also inversely as the time in which it passes

or in other words, directly as the velocity. But what is to create inequality of velocity when the channel, a wire for instance, is of the same size and nature in both cases? When the same fluid is in question, the velocity will be as the forces by which it may be impelled. The only forces to which electricity has ever been alleged to be liable, as far as I am informed, are either the self-attractive or self-repulsive power of its own particles, or their attraction for other matter. It will be admitted that the intensity of these forces must, in the case of electricity, as in that of caloric or light, be as the quantity to the containing space; and consequently that it would be unreasonable to allege that the reciprocal repulsion of the electric atoms, or their attraction for other matter, and consequently any velocity thence arising, should not be in proportion to the state of condensation from which they may be liberated.

If the superior velocity displayed by electricity generated by friction be the cause, not the consequence of greater intensity, how are we to account for the superior velocity?

According to the doctrine of Du Fay, electricity is retained upon the surface of a charged pane by the reciprocal attraction of the heterogeneous fluids. According to the Franklinian doctrine it is retained by its attraction for the negative surface, on which side this attraction is not counterbalanced by repulsion from other electricity. When the circuit is completed by a conductor, according to the one doctrine, a surcharge on the one side is translated to the other; while, according to the other doctrine, two heterogeneous fluids rush from the surfaces in which they are previously accumulated in excess, to enter into combination, and, at the same time, to restore the equilibrium of the surfaces on which they have been respectively deficient. But according to either hypothesis, wherefore should the forces be greater for electric matter when generated by one means, than when produced by another? Why should a mass of electric matter evolved in a diffuse state by a machine or from a cloud, rush like a bullet through conductors which are almost impassable to the same fluid when evolved from a state of extreme density within a simple galvanic circuit?

During the process of exciting an electric battery, the electricity previously existing equally on both sides of the glass is so transferred from one side to the other, that the one becomes as much negative as the other becomes positive; and it must be evident that the intensity will be limited by the extent of the force by which this transfer is effected. It is difficult to conceive that merely by a change of capacity arising from friction, a force should be gene-

rated at all comparable to that which the electric matter must exert, in escaping, according to the premises, from a state of extreme density, as when extricated by galvanic action from water or zinc.

I ascertained, some years ago, that the galvanic fluid evolved by a large calorimotor of a single pair, will not ignite a wire which may be easily deflagrated by a much smaller apparatus of the same construction. Yet sheets of metal about four inches in breadth, might be raised by a discharge from the larger instrument above the temperature of boiling water. In such cases, agreeably to the doctrine of quantity and intensity, the electric fluid exists up to the period of its evolution, in a state of extreme condensation and consequent intensity, and yet at the moment when a perfect but restricted channel is afforded to it, becomes too diffuse to pass through it with a velocity sufficient to produce deflagration. How can the electricity which is in the one case so dense, become in the other so rare? Where, and in what manner does it exist intermediately between the period of its condensation within the pores of the generating materials, and its rarefaction in the wire which forms the circuit between them?

I am aware that to the want of adequate insulation, the inferior intensity of the charges communicated to coated surfaces by voltaic apparatus, will be attributed; as it cannot, without a palpable contradiction, be ascribed to any defect of intensity, in a source wherein the ratio of the quantity to the space is almost infinitely great.

Let us, then, examine the subject agreeably to this view of the case. Since the electricity liberated by electro-chemical reaction by means of a single galvanic pair must have pre-existed in a state of extreme condensation and consequent intensity, it follows that it ought to be productive of a tension limited only by the insulating power of the menstruum within which it is extricated. It should, then, when evolved as above described, attain the highest degree of intensity consistent with the insulating power alluded to. That this is not the fact, is fully established by general experience, and by the observations of Faraday, according to which the intensity of a voltaic series increases with the number of pairs employed.

It results also from the premises, that the tension should become as great in a large as in a small pair; and by employing one large pair, effects should be attainable, as potent in respect to intensity, and more potent as respects quantity, than those resulting from a series of pairs. Yet the experiments above-mentioned prove that as the surfaces associated as a single galvanic pair are enlarged, the

intensity lessens; so that a calorimotor of a single pair containing fifty square feet of zinc, will not, in a wire of any size, produce an ignition of as high intensity as may be effected by the elementary battery of Wollaston, formed of a silver thimble, and piece of zinc proportionably minute.

It appears from the experiments of Professors W. B. and H. D. Rogers,* that the power of a galvanic pair in deflecting a magnetic needle, was increased by causing the surface of the copper plate to exceed that of the zinc; while by extending the zinc surface beyond that of the copper, little or no increase of power ensued. This result appears to be the opposite of that which the theory of Wollaston, supported by some recent observations of Faraday, would lead us to expect. As pursuant to that theory, the galvano-electric fluid is due exclusively to chemical reaction; if the charge were not promoted by an excess of extension in the oxidizable metal, it ought not to have been improved by similar extension of that which is insusceptible of oxidizement.

According to the observations of the Professors above mentioned, the deflection resulting from a galvanic discharge, on the first immersion of the plates, after a repose of two hours, was six times as great as that which could be permanently sustained. The greatest effect appeared always to ensue before there was any sensible extrication of hydrogen, and the commencement of the effervescence was invariably the signal for a decline of power.

It was by analogous observations respecting the igniting influence of galvanic apparatus that I was led to the construction of my deflagrators†, in which the deflagrating power appears, agreeably to my experience, to be exalted as much by the repose of the surfaces as the ability to influence the magnetic needle was ascertained to be, in the experiments of my sagacious friends above mentioned.

That the evolution of the galvanic fluid is not in proportion to the intensity of the chemical reaction, is corroborated by the fact, that the intensity of the ignition, excited in a wire by a galvanic discharge, diminishes, while the effervescence increases; and it is well known that the power of galvanic apparatus is not augmented by adding to the strength of the solvent, beyond a very moderate limit.

But if it be granted that chemical affinity, when reacting within a galvanic circuit, without any propulsive power from the elements of the circuit, can receive a peculiar

* See Silliman's Journal for October, 1834.

† Dr. Hare's deflagrators will be described in our next number. EDIT.

impulse, so as to produce a current of the electric fluid, confining it at the same time to a very narrow channel, by what process can this species of chemical reaction be conceived to accelerate an electrical current already produced? The same propulsion must be given to the electricity liberated between the plates of the second pair as between those of the first; and it is inconceivable to me that the accession of that derived from the first pair should add to the velocity of the portion evolved by the second. The current of the former cannot be supposed to move with greater velocity on account of its meeting with another, which moves at its own rate. Currents are not accelerated by their confluence, unless the head or pressure be increased and the channel restricted. But in the case in point it has been shown, that by the reaction of the solvent with the first pair in the series, the tension must attain the highest degree consistent with the imperfect insulation; and no cause has been assigned for the restriction of the channel. It may be said that the current from the first pair cannot pass through the liquid in the second cell without causing the decomposition of that liquid; and that as its power is inadequate to effect this change, it has to pursue the same route as the electricity which is generated by the oxidizement of the second plate of zinc. It is still difficult to me to imagine that it can transfer its momentum to the current which thus precedes it; or, that the chemical reaction by which the latter is evolved, should act only in accelerating the stream which it receives from the preceding pair.—Granting that imponderable matter, at the moment of its extrication from confinement among ponderable atoms, were to receive an impulse which, by extraneous co-operating causes, should force it to move in a current, yet I cannot imagine that such atoms can, by any reaction originating between themselves, give an impulse to imponderable matter extricated from other atoms. Whether or not the electricity be derived from chemical reaction, it seems to me that the power which puts it in motion, and accelerates and condenses it into a channel, still smaller and smaller as its intensity increases, must be ascribed to some mysterious property arising from the arrangement of the elements of the series, which is in the present state of our knowledge, inexplicable.

This electromotive power, if not antecedent, does not appear to me to be consequent to chemical reaction. I conceive that it operates upon all the imponderable elements within its scope, tending to accumulate them at the "electrodes" under a greater or less degree of tension.—The potency of the resulting discharge, when the circuit is

completed, is regulated both by the tension and the quantity of the imponderable matter accumulated. But the presence of reagents, which favour the extrication of imponderable materials, as in the more efficient voltaic apparatus, is compatible only with a feeble insulation, while arrangements more favourable to insulation as in De Luc's Electric column, are incompatible with a copious supply of the imponderable matter.

Probably upon an analogous ability to produce or annul, to promote or retard, chemical reaction, the efficacy of of animal and vegetable organization is founded, being obviously dependent on an arrangement of masses. The voltaic series of a gymnotus is evidently an animal organ, and its analogy with the voltaic series produced by human ingenuity induces me to consider the latter in the same class of agents as the organs by which life is supported.

I should have expected, that in establishing the highly interesting fact that every elementary equivalent has the same quantity of electricity, the ingenious author of this discovery would have adverted to the analogous observations of Petit and Dulong respecting the specific heat of elementary atoms. It strikes me as important, that similar conclusions should have been arrived at by such high authority, both as respects caloric and the electric fluid. I am surprised that Faraday should appear to have overlooked this analogy in the explanation of the practical results which he has obtained.

No hypothesis appears to be more generally sanctioned at this time among chemists than that which ascribes the aeriform state to a union between caloric and ponderable matter. When hydrogen unites with oxygen, caloric is evolved. It follows that when these substances are made to resume the gaseous form, caloric must be supplied to them.

When it is considered that the inferences of Petit and Dulong, respecting the specific caloric, and those of Faraday respecting the electricity combined with ponderable equivalents, tend to demonstrate the coexistence in them of equivalent atmospheres of each of those imponderable fluids, does it not authorise a surmise that in the voltaic current they may be associated; and that with those equivalent measures of electricity which Faraday has shown to pass, corresponding portions of caloric are imparted? The idea of Berzelius, "*that the heat and light evolved during powerful combinations, are in consequence of an electric discharge at the same moment taking place*" being cited by Faraday in the language of this quotation, he observes, that it "is strictly in accordance with his view of the quantity of electricity associated with the particles

of matter." To me it appears to be no less in accordance with the idea that heat and light are associated with those atoms to a commensurate extent; and since, by the premises, electricity re-acts with them, they may be presumed to react with electricity.

That heat, light, and electricity are all concomitant products of electro-chemical re-action, is self-evident. Agreeably then, to the strict rules of induction, wherefore is the principle last mentioned to be considered as the cause of the others?

Where is the proof that the heat and light evolved between the "electrodes" are effects merely of electricity? The fact of the apparently unlimited evolution of heat from a finite portion of wire duly subjected to a voltaic circuit, is inexplicable, consistently with the materiality of caloric, unless we suppose the fluid to be derived from the same electro-chemical reaction to which we owe the electricity associated therewith.

I conceive it to be almost self-evident, that mechanical and voltaic electricity are due to the same fluid, so far as they are strictly electrical. The only doubt with me is, whether the very different characteristics of the phenomena produced by the different means alluded to could be explained without supposing some other modifying causes. And at all events, from the reasons above given, I am dissatisfied with the explanation that the difference is dependent on quantity and intensity.

In terminating my observations, I subjoin the following statement of my opinions as heretofore expressed in one of my text books.

"It does not appear to me that the production of electro-magnetic phenomena, both by galvanic and by electrical discharges disproves my opinion, that caloric and electricity are connate and co-ordinate products of galvanic action.

As ignition is producible by either discharge, whether electric or galvanic, the fluid of heat, no less than the electric fluid, may in both cases be concerned; and it is yet to be shown, that magnetic phenomena are ever due to the unalloyed agency of electricity.

It is true that magnetism has been imparted, by discharges of mechanical electricity, without any ostensible agency of caloric; but it is equally true, that magnetic movements have been produced also by the application of heat, unaccompanied by any ostensible agency of the electric fluid, and it seems as rationable to suppose that caloric and electricity are associated in the first instance as in the last.

Those who consider electricity, varying in quantity and

intensity, as the common cause of electrical and galvanic ignition, and of thermo-magnetic phenomena, must suppose that this principle and caloric are capable of a reciprocal action. In the first case, caloric is evolved by electric action; in the last, electric currents are produced by calorific repulsion. Hence, as action and reaction are equal and contrary, I deem it rational to suppose that in some cases the former, in other cases the latter, may be the prime mover; but that both participate in every galvanic, electro-magnetic, or thermo-magnetic current.

VII. *On Electro-pulsations and Electro-momentum. By* WILLIAM STURGEON, *Lecturer on Experimental Philosophy at the Honourable East India Company's Military Academy, Addiscombe,* &c.*

It is very well known to the readers of the Philosophical Magazine, that I have long considered electric currents, when transmitted through inferior conductors between the poles of a voltaic battery, as the effect of a series of distinct discharges, in such rapid succession as not to be individually distinguished by the senses. Such currents I have called electro-pulsatory. See my theory of magnetic electricity in the London and Edinburgh Philosophical Magazine, vol. ii. p. 202.

By following up these views of electro-pulsations, I was about two years ago enabled to dispense with all acid or saline liquids, in the employment of galvanic batteries, for the purpose of galvanizing, as it is called, either to satisfy the curiosity, or as a medical process; and my plan which answers very well, I have found to be productive of a considerable saving in the expence necessarily attendant on the use of voltaic batteries when excited by acid solutions.

It is well known that a Cruickshank battery of about a hundred pairs will, by employing water alone in the cells, charge to a certain degree of intensity almost any extent of coated surface of glass that we please; and that the same degree of charge is given to it by a single contact of the conductors, however short its duration. This being understood, and understanding also that the shock produced by any discharge from a given intensity would be proportionable to the quantity of fluid transmitted in a given time, it was easy to foresee that a series of shocks in rapid succession might be produced by some mechanical contrivance, and that the degree of force might be regulated by varying the extent of coated surface.

* From the London and Edin. Philos. Mag. &c &c. for Aug. 1836.

My first experiments were made with a hundred and fifty pairs of three-inch plates, and about seven feet on each side of coated glass; and my apparatus for producing a rapid succession of shocks was one of Mr. Barlow's stellated electro-magnetic wheels* which was soldered to an iron spindle and put into a rotatory motion by a wheel and band; the points of the wheel touching in succession a copper in connexion with a positive surface, and thus producing a discharge at every contact of the wheel and copper spring.

When the two surfaces are connected by wires with two basins of salt water, and the hands immersed one in each basin, the effect experienced is precisely that of the discharge of a voltaic battery. The discharges can be made in such rapid succession as to prevent the sensation of distinct shocks; and if the process were to be concealed it would require some experience to distinguish between the effects on the animal economy, from this apparatus and those from a voltaic battery charged with acid and water.

My views being so far verified, the next attempt was to simplify the apparatus and make it more portable; and as it was readily seen that if one hundred pairs would charge glass of considerable thickness, thinner glass might be charged by fewer pairs; this was done, and eventually the glass entirely dismissed, and its place supplied by well-varnished Bristol-board. These boards answer exceedingly well as a reservoir for low intensities; they may be coated to within an inch of the edge all round, and placed upon their edges either on a piece of glass or on a board properly prepared, and arranged to any required extent like the plates of a voltaic battery, but when considerable intensity is wanted it is better to use thin glass.

From these facts we learn that metallic surfaces of many acres of extent may possibly be charged to a low intensity in the interior of the earth, by having a thin intervening stratum of inferior conducting matter sufficient to insulate from each other their dissimilar electric surfaces.

It may now be understood that the slightest accident which would suddenly break through the insulation, such as the sinking of a mass of metalline matter from one stratum to the other, would cause a sudden rush of an immense ocean of the electric fluid, which might be productive of subterranean lightnings and tremendous explosions sufficient to shake an extensive range of country on every side.

Connected with the preceding facts there are others which may be conveniently mentioned in this place, and which would lead us to similar explanations of the causes of subterraneous convulsions. Electric currents of considerable magnitude when suddenly checked or diverted to a new chan-

* Mr. Barlow's "Magnetic Attractions," Second Edition.

nel, produce a momentum not very generally understood; but which I will endeavour to explain. A coil of copper wire excited by magnetic action will become a channel for an electric current; and whilst the whole circuit is metallic, the velocity of that current would be considerably greater than if any, even a small part of the circuit were of worse conducting materials; and if the current were suddenly transferred from a channel of the former character to one of the latter, by any contrivance whatever, it would meet a resistance on entering the new channel, which the momentum it had previously required would have to overcome; and a sudden disturbance of the electric fluid, previously at rest would take place, and a violent rush of the current would as suddenly follow.

It is in this manner that shocks and sparks are produced by magnetic electric machines, where the current, previously in rapid motion, is suddenly transferred to a new channel of inferior conducting matter; and all the fluid in the revolving coil rushes through a person properly situated for the new route, and who experiences the electric shock, or else through a thin stratum of air at an interruption in the metallic circuit where the spark is produced.

These, then, are some of the effects of electric currents, or of the momentum of the electric fluid in a state of motion after the exciting cause is entirely cut off. The shock thus produced may very conveniently be compared to the blow given by Montgolfier's hydraulic ram. Electro-momenta may be produced by any mode of excitation whatever, and the effects will be proportional to the velocity and quantity of the electric fluid first put in motion; and the length of the original channel is also to be taken into account. If then electro-momenta, capable of producing violent shocks and vivid sparks can be produced by a few hundreds of feet of thin copper wire, what is it that might not be expected from the electro-momenta of nature, arising from currents of many miles in extent, kept in motion either by heat, saline solutions, or by other causes, amongst the metalline strata below the surface of the earth? A sudden disruption in the circuit would insure the blow, and an earthquake might be the result.

Artillery Place, Woolwich, July 4th, 1836.

VIII. *An inquiry into the Attributes of the Galvanometer, and how far its indications may be depended upon in Electro-dynamic researches.*

The diversified capacities in which the galvanometer has found employment in the service of experimenters, and the interesting discoveries which have been attributed to its

assistance, have conferred on it an importance of no ordinary kind. It is an instrument now extensively employed in the contemplation of almost every class of phenomena exhibited by the agency of electric currents: and the deflections of its needle have been regarded as infallible indications of the *degree* of electric force in its wires;—of the *absolute quantity* of the electric fluid transmitted by them;—and of the extent of electric agency in the production of chemical action.

That the deflections of the magnetic needle are strictly indicative of the electric *force*, or electro-dynamic action, when contemplated abstractedly, and independently as regards the elements of its constitution, is a truth duly acknowledged, and sanctioned by the highest scientific authority: but we have not yet been made acquainted with any experiments, nor with any train of reasoning, from which we could infer that its powers of investigation have been any farther established.

There are two distinct kinds of magnetic deflections, for the admeasurement of electro-dynamic action, which may very conveniently be designated *permanent* and *transient*. By the former we mean all those deflections of the galvanometer needle, which are steadily maintained for some considerable time, constantly marking some determinate angle with the conducting wire, according to the extent of the electric force which keeps the needle out of its natural direction. And by the latter, we mean those momentary deflections, or first ranges of the needle, which are the effects of sudden electric discharges, operating for some definite interval of time; the angle indicating the extent of the electric force, being that marked by the extreme range of the needle.

These two kinds of magnetic deflections are perfectly distinct from each other, and indicate the values of electric forces by very different laws, well known to scientific men; some of whom have successfully applied them to particular electro-dynamic investigations. It still however remains a subject of interesting enquiry, whether or no, the initial and subsequent electric forces observe an uniform analogy with regard to each other; or, if, in all cases of excitation any constant ratio of those forces be indicated by the *transient* and *permanent* deflections of the galvanometer needle, whilst under the influence of electro-dynamic action. If such analogy were to be established, it would be a means of abridging our experimental labours, and satisfying many of our enquiries by a simple process of calculation alone. We might then *substitute*, as it were, one experiment for another, and become acquainted with

the values of electric forces emanating from various sources of action to which we had no immediate means of access; or if we had, it would be unnecessary to resort to them.

If, for instance, it were discovered that the forces indicated by a series of steady deflections, at intervals of one degree, through a quadrant of the graduated card, observed a constant ratio with regard to their respective initial forces, acting for some small interval of time; a law would thus be established by means of which the values of either the initial, or the subsequent permanent forces might easily be calculated.

We presume not, however, from the results of our own limited investigations, to hold out even a hope of arriving at conclusions of this high importance to electrical science; but as *substitute* experiments, implying such analogy, appear to have found admission into some electro-dynamic researches hitherto held in considerable repute, and from which circumstance others might possibly be assumed, our conclusions, whatever they may be, if drawn from a well conducted enquiry, may probably be of some value to those who pursue similar paths of research, either by leading to truth, or preventing error.

In the contemplation of the electric forces indicated by *permanent* deflections, the resistance which might be presented to a needle in motion, by the surrounding air, may be entirely disregarded; because, whilst under the influence of the electric force, the needle is supposed to be in a perfect state of repose. The only force, therefore with which the electro-dynamic action has to contend, is that arising from the propensity with which the displaced needle is endued, of resuming its former or natural position: and when the same galvanometer is employed, this directive force is invariable, or is a constant quantity, which may be referred to unity.

The deflecting force exhibited by the conducting wire during the flow of an electric current, is well known to operate at right angles to the axis of that wire, around every part of its surface: or in the direction of a tangent to any point of the curve circumscribing a transverse section of the wire, as shown in figs. 1, 2, 3.

The efforts, therefore, of that force to turn a magnetic needle placed parallel to, and either above or below the wire, will be in the direction of a tangent to the horizontal arc of deflection; and the force has very conveniently been estimated in terms of the tangent to that arc, being constantly proportional to it.

If therefore, the angle of *permanent* deflection be denoted by Δ, then the force in the conducting wires producing that

angle, will be proportional to tan. Δ. And by employing F as a representative of the force emanating from the conducting wire, we have

$$\tan. \Delta = F.^{*} \ldots\ldots (a)$$

Electric forces, indicated by *transient* deflections of the needle, are but of very short duration, and in some cases may be regarded as momentary impulses, producing velocities in the needle's motion proportional to their intensities. If then, the maximum velocities were to be produced at the lowest point of the measuring arc, or whilst the needle and conducting wire were parallel to each other, the electric forces, producing these deflections, would be calculated with tolerable precision, upon the principles of the doctrine of falling bodies, the deflections or the needle being assimilated to pendulous motions. It appears, therefore, an exceedingly desirable object in calculating the values of transient electric forces, that the velocity should be a maximum at the lowest point of the measuring arc; hence in experiments of this kind, every available means should be resorted to, in order to attain the greatest degree of precision in that particular. It would be exceedingly difficult however, if not entirely impossible, to produce a maximum velocity at the precise zero point, which could be accomplished by no other means than by adjusting the needle. previously to the discharge, some few degrees backward, or on the oposite side of the meridian line to that of the measuring arc. Perhaps two or three degrees might answer as well, if not better than any other distance for electric forces generally of about half a second duration; though much would depend upon their intensity, and other circumstances, which precludes the accuracy of any general rule. For our present purpose, however, we consider, this mode of experimenting sufficiently accurate.

We now consider the needle as leaving the conducting wire, or lowest point of the measuring arc, with a maximum velocity, and consequently with the same velocity as that which it would acquire whilst returning from the highest point to its parallelism with the wire again, by that force alone which tends to restore it to its natural position.

Under these circumstances, the electro-dynamic action. for any short definite interval of time, will be proportional to the chords of the arcs of deflection, or to twice the sines of half those arcs. Denoting therefore the arc of *transient* deflection by δ, and the electric force producing that arc by F′, we shall have

$$2 \sin. \tfrac{1}{2} \delta = F' \ldots\ldots (b)$$

*See Mr. Barlow's "Magnetic Attractions" 2nd Edition, p. 238.

Electric forces producing *permanent* deflections may be regarded as if emanating from an equable flow* of the electric fluid through the conducting wire in each case, from the first moment the needle marks a steady angle; and that an equal quantity is transmitted in each equal interval of time. If therefore, the flow of the currents were equable from the first closing of the circuit, it is obvious that the force would be constant from that moment; in which case the initial forces indicated by *transient* deflections ought to be proportional to those indicated by *permanent* deflections. That is, when steady forces shown by permanent deflections, from different sources are equal, the initial increments of those forces ought to be equal also. Or if the steady forces be unequal, still the initial incremental forces shown by transient deflections, ought to be proportional to them. So that if Δ and Δ', donote the angles of steady or *permanent* deflections, from two different sources of excitation, and δ and δ' the corresponding angles of *transient* deflection, produced by sudden discharges, each acting for some small standard interval of time, from the same sources respectively, then by taking our former values of the electric forces in terms of the angles of deflections, we ought to have

$$\frac{\tan.\ \Delta'}{\tan.\ \Delta} = \frac{\sin.\ \frac{1}{2}\delta'}{\sin.\ \frac{1}{2}\delta} \text{ :......(c)}$$

and when $\Delta = \Delta'$, so should $\delta = \delta'$.

But in electric discharges generally, it is a well known fact, that the initial forces are superior to those subsequently exhibited in any similar interval of time; provided no greater a degree of excitation be carried on during the discharge, than at or before its commencement. This fact is particularly applicable to the discharge of a Leyden jar, the *force* of which depends upon the elasticity of the fluid superinduced on its surface. The elasticity in this case is proportional to the *density* of the fluid on the coating; and as the density is in its most exalted state at the commencement of the discharge, it follows, that the *initial* force is superior to the force taken at any subsequent period of the fluid's motion; and, in consequence of the progressive diminution of density in the jar, the force in the circuit necessarily lessens proportionally, from the first to the last moment of transmission.

The *velocity* of the fluid in the circuit, is also dependent on its elasticity in the jar. Hence it is, that the initial

* We are very well aware that a magnetic needle may be kept perfectly steady by a rapid succession of impulses of equal intensity which could not be strictly called an "equable flow" of the fluid, but even in that case equal quantities would be transmitted in equal intervals of time.

velocity is a maximum in every discharge from a jar. And for the same reason, the *quantity* transmitted in the first interval of time, is greater than in any other similar interval during the discharge. This is universally the case, whatever may be the size of the jar, or with whatever quantity of fluid it may be charged.

It is obvious also, that the initial force from any, the same standard *quantity* of fluid, will vary with every variation in the extent of the coated surface over which it is distributed; for the initial force in the circuit depends upon the elasticity of the charge, and the elasticity upon the density. But the density varies in the inverse ratio of the extent of coated surface over which the fluid is distributed; hence, the initial force of the same quantity varies with every variation in the extent of coated surface.

These circumstances are inseparably connected with the discharge of electric jars, whatever be the nature of the conductor through which the fluid is transmitted, provided its conductibility suffers no change during the process.—But the *effects* absolutely produced, and the *time* of transmission, will be modified considerably by the character of the conductor.

The most protracted discharge of a Leyden jar, however, is too transient to command even a moment's steady deflection of a magnetic needle. On this account, therefore, as well as in consequence of an unequable flow of the fluid during the transmission, *permanent* deflections are absolutely unattainable; and consequently the admeasurement of electric forces by those means, entirely out of the question: and the action of a current directly from the machine has never yet been shown to afford the necessary data.

Thus circumstanced, we are necessarily compelled to abandon all hopes of ever arriving at experimental results, from the electrical machine, that could at all favour the idea of lessening our future labours by electro-dynamic ratios of permanent and transient action. Notwithstanding however, the impracticability of obtaining *permanent* deflections from the discharge of a jar, or from currents direct from the machine, they are very easily accomplished by availing ourselves of other sources of electric action, which, fortunately for enquiries of this kind, are both abundant and of easy access. It may therefore not be amiss, in this part of the discussion, to make ourselves acquainted with a few experimental results from various sources, which are at immediate command. And in order to obviate any obscurity which might happen to occur from a want of uniformity in the arrangement of the experiments, we have been led to a selection of a standard angle

of five degrees for the permanent deflections, from whatever source they may proceed. These are all arranged in one vertical column in the following table; the transient deflections are placed in the same lines in a column on the right, and the respective sources of electric action are seen on the left. One second of time was allowed for the electro-dynamic action to produce the transient deflections.

The galvanometer, employed in these experiments, has a rectangular coil, each side of which is six inches; and consists of eight convolutions. The needle which is four inches long, and furnished with an agate cap, is placed in the centre of the vertical coil on a fine steel pivot. And a thread of sewing silk, with a brass ball at its lower end, constituted the second's pendulum. The metals employed in the voltaic experiments were a strip of copper and another of zinc, each one inch broad and placed about a quarter of an inch apart. The thermo-electric apparatus was a thin bar of bismuth with a copper wire at each end.

Table of Permanent and Transient Deflections of a Magnetic Needle by Electric currents, from various sources of excitation.

Source of Electric Action.		Deflections.	
Copper and zinc area of surfaces immersed.	Exciting Liquid.	Permanent.	Transient.
Square inches.			
4 . 0	Water and sulphuric acid.	5°	15°
6 . 0	Weaker acid solution.	5°	12°
0 . 6	Stronger solution than 1st	5°	10°
4 . 0	Water and muriatic acid.	5°	13°
0 . 6	Stronger acid solution.	5°	10°
4 . 0	Water and nitrous acid.	5°	10°
0 . 2	Strong solution.	5°	11°
6 . 0	Commom salt & water with a few drops of nitrous acid	5°	47°
Bar of bismuth & copper wires.	Excited by a Spirit Lamp.	5°	9°

The tabular transient deflections exhibited in the third column, although they do not express a close correspondency among themselves, appear to have an approximate relation to the permanent standard deflection, which might perhaps be more fully established by means of extensive series of experiments. The mean of all the first seven deflections would be $11\frac{7}{11}°$; and the permanent to the mean transient as 1:2·346 nearly. But if we compare experiments 1, 4, and 6, in which the same extent of metallic surface was employed, a considerable discrepancy is immediately seen: the extremes differing five degrees, or half the quantity of the lesser deflection; so that as far as regards the experiments in which acid solutions were employed, very little in common is perceived amongst their results. There is however in these first seven experiments one striking feature which is deserving of notice—which is that the angles of transient deflection are not less than twice that of permanent deflection. The ninth, or thermo-electric experiment also appears to approach the same law. Other galvanometers, and other intervals of time for the action of the transient force have, however given very different results. The eighth experiment in which salt and water were employed, exhibits a very different law to any of the rest, the transient deflection indicating an electro-dynamic action of about three times the intensity of that of the greatest of those with acid solutions only.

Hitherto the formula, *c*, has been considered as applicable to those cases only in which the electro-dynamic action is supposed to be constant from the first closing of the circuit. But it would be equally applicable whether the initial and subsequent actions were uniform or not, provided they exhibited a constant ratio in all the individual currents under contemplation. For if F' and F represent the permanent forces of two currents, and f' and f their respective initial forces, then since

$$F' : F :: f' : f \qquad \frac{F'}{F} = \frac{f'}{f}$$

and by substituting the respective values of those forces, in terms of the arcs of deflection, we have

$$\frac{\tan.\ \Delta'}{\tan.\ \Delta} = \frac{\sin.\ \frac{1}{2}\ \delta'}{\sin.\ \frac{1}{2}\ \delta} \qquad \text{the same as before.}$$

There are only three cases, however, in the tabular results in which this ratio is exhibited, viz. the third, fifth, and sixth: in those it is shown that the permanent is to the

transient as 5 : 10, or, as 1 : 2. All the rest exhibit different ratios, no two of which are alike. And although our experimental enquiries have been pursued to a considerable extent, and conducted with great care, employing different inrervals of time, from half a second to three seconds, for the play of the initial force, the prospects they have unfolded towards arriving at the analogy sought for, are no more promising than these exhibited in the table.

With regard to Voltaic arrangements, the action on the needle varies with almost every circumstance which enters the process of experiment; and the exact* ratios of permanent and transient forces which they develop, are probably as exhaustless as the variety of particulars both intentional and incidental, that happen to be connected with the process. The character of the exciting liquid,—the character and extent of the metallic surfaces,—their state of oxidation,—their distance from one another,—the time the circuit has been broken—are amongst the most obvious of the probable multitude of circumstamces which influence the initial electro-dynamic action.

The sources of electric action which appear the most favourable for developing uniform results, are those in which *heat* is the exciting medium. The circumstances connected with this process being very limited, the experiments are not so liable to be influenced by casualities as those in which liquids are employed.

The difference of opinion which prevails among scientific men, regarding the indications of the galvanometer as a measurer of the "absolute quantity" of electric fluid developed by various processes of excitation, may possibly arise from a want of due attention to the *modus operandi* of transmission. Perhaps a few hints may tend to develop other views of this subject, than those which have hitherto been taken, and to reconcile the apparent discrepancies with which the phenomena are attended. They may very properly be prefaced by an

Experiment. A pair of copper and zinc cylinders the former exposing about sixty, the latter about twenty square inches of surface to a strong solution of salt and water, had been in electrical play for forty-six hours, at which time the permanent deflection was reduced to 2°. The circuit was now broken (without disturbing the metals,) for ten minutes; then closed for one second of time; the needle was driven through an arc of 75°.

* We say "exact ratios" because when very light needles are employed perhaps no two would be found precisely alike. Those results which are placed in the table were arrived at by using a comparatively heavy needle.

It would be exceedingly difficult to make it appear that this voltaic pair, or indeed any other of moderate size, could hold in suspense, a charge of the electric fluid greater than that which could be given to an extensive surface of glass, by the common machine: yet, perhaps, it would be as difficult to show, that the transmission of an intense charge of fifty square feet of coated surface would produce a similar effect on the same galvanometer. Nothing of the has hitherto been done; neither are we aware that any experiments, or any train of reasoning have yet been attempted, which would satisfactorily explain the obvious difference of the galvanometer indications exhibited by discharges from the machine, and those from other sources of electric action.

The "absolute quantity" of fluid put into motion by a voltaic pair, may possibly be very small, when compared to that constituting the charges of a Leyden jar of even moderate dimensions, and yet, its electro-dynamic action on the needle might be much greater than that of a discharge from the jar; because, in the former case, the fluid would circulate a multitude of times in a comparatively compact body, whilst in the latter case (the same time being allowed) the charge would be drawn out, as it were, into a thin slender stream, made up of an infinite number of small parts of the "absolute quantity" or whole charge.

In the one case the effect due to a certain quantity of fluid is reproduced with infinite celerity, which operates on the needle as a series of distinct discharges succeeding one another with sufficient rapidity to produce a deflection which would indicate a force equal to the sum total of the series: or equal to the momentum arising from the number of circumvolutions performed by the fluid in a given interval of time. If, then, the effect due to one transit of the "absolute quantity" be represented by m, and the number of repetitions in the series by n, the momentum on the needle in the first interval of time would be $n\ m$. If we suppose the number of repetitions, of the unit of force, in one second to be $1000=n$, then, whatever be the value of m, we should have the momentum, or whole electro-dynamic action on the needle, in one second, equal to $1000\ m$. Or, the effect on the needle would be that of a thousand single discharges of the standard or "absolute quantity" of fluid. But if the same "absolute quantity" be discharged from a jar, and that it requires one second of time for its transmission through the galvanometer coil, we can have no more electro-dynamic action than that produced by the sum total of its parts, (whether they be equal or unequal,) once produced: or by one solitary transmission, of the

"absolute quantity," from the positive to the negative sur face.

If we be permitted to conduct our reasoning in this manner, we shall obviously be led to understand that the "absolute quantity" of fluid in motion, separately considered, may possibly perform but a very insignificant part in the production of magnetic deflections, and must at all times be but very imperfectly represented by the galvanometer.

This explanation is very different to that which supposes that "four grains of zinc, with one of water may yield as much electricity as is evolved during a thunder storm;" but as to which of them is to be indulged with the greatest degree of *honest* favour from the philosophical world, will require time to determine.

(To be resumed.)

IX. *"On the Relation by measure of Common and Voltaic Electricity," in a letter from* Mr. Sturgeon *to* Dr. Faraday.

Sir,

Your "Researches for discovering the Relation by measure, of Common and Voltaic Electricity" appear to have been considered as a necessary preliminary for the establishment of a "theory of chemical decomposition." In these "researches" a few experiments are described, the results of which you seem to have regarded as sufficient data whereon to found certain positions on which the "theory" mainly rests.

My views of the method by which you have pursued this part of your "researches," although never before published have been known for a long time, to some of the most illustrious of the present day, in experimental science: but, perhaps, with this exception, the results of these your enquiries have never yet been questioned; but, on the contrary, I believe, have generally been held in very high esteem.

A close examination of your third series, which I hope was conducted with great care, soon induced me, however, to entertain very different opinions to those generally expressed regarding the *basis* of your theory: and as I think it possible that in this case, as in one on a former occasion, my sentiments may have been made known to you in a very distorted form, I consider that it is but just that I should publish the results of my examination of your "Relation by measure, &c." in the most unsophisticated and undisguised manner. And as I know of no experi-

menter of the present day more ardent in these interesting enquiries than you are; nor of any one more fortunately situated for stamping his investigations with imperishable fame; so I know of no one to whom I can address with a greater degree of propriety the results of my examination, nor of whom I can make a more suitable selection, to reconcile to scientific order, those prominent inaccuracies which I have been led to discover in the first grand step of your theoretical design, and which I am now about to offer to your notice.

The first point you have attempted to establish is, "*that if the same absolute quantity of electricity pass through the Galvanometer, whatever may be its intensity, the deflecting force upon the needle is the same.*" (Articles 366, 367.)*

This inference appears so exceedingly vague and inconclusive, that it cannot, with propriety, be said to express anything: or if it has any definite meaning, I must confess that I cannot discover it. The intensity of any given quantity of the electric fluid whilst passing through a given conductor, will be inversely as the time it occupies.—Therefore if the intensity be of no consequence, neither can the time be of any. Hence according to your inference, it is a matter of indifference whether "the same quantity" occupies a second, a minute, a month, or a year in passing "*through the galvanometer, the deflecting force upon the needle is the same;*" a conclusion which I am willing to believe you never intended as the true meaning of the principle you were endeavouring to establish, but which you will readily perceive, is a fair deduction from the language in which the position is expressed.

The experiments which are described as leading to this remarkable inference, appear equally unsatisfactory; for notwithstanding all the care that might be taken in keeping the machine in constant uniform action, it is very obvious that the discharges did not all proceed in the same manner through the galvanometer. The "latteral transmission in the galvanometer coil," the possibility of which is acknowledged to have taken place in some, if not in all the discharges, (article 365) would alone be sufficient to disqualify the experiments for any investigations of this high importance. The galvanometers usually employed, and which answer admirably for Voltaic electricity, are very unsuitable for ascertaining the exact amount of deflection due to discharges of electric jars, and more particularly so when those jars are charged to high intensities, as in those

* The articles referred to are those in Dr. Faraday's paper, the most essential of which will be found appended to this letter.

experiments described in article 363; for when the convolutions of the coil are packed close together, the fluid invariably springs from one to another, and does not proceed in that tranquil uniform manner from one end of the wire through its whole length to the other, as in discharges of voltaic electricity. It more frequently happens that every part of the coil, silk, &c. exhibits a considerable degree of electric tension during the transit of heavy discharges; and the effects due to that influence are often very troublesome, and not easy to avoid.

I am very well aware that the precautions against error which you have described, and the correspondency which appears in the results of those few experiments which you have chosen as favourable to your views, are well calculated to captivate the credulity of the unexperienced in electricity; but that apparent exactness which would prevailingly command *their* ascent, might possibly be the very means of suscitating the electrician to a closer examination of the process by which such precision was obtained. Whether it was, or was not, an influence of this kind which first excited my curiosity to an examination of your paper, is now a matter of no consequence; I have long been sufficiently interested in the subject to induce me to pay particular attention to every novel fact and opinion that have appeared to me to be either directly or indirectly connected with it. I have, therefore, not read your third series either supinely, or partially, but have perused the whole and collated its parts with great attention. I have however failed to discover that degree of correspondency in the results of your experiments and the inference you have drawn from them, which any one would have been led to expect; but on the contrary, I have met with some discrepancies from the interposition of which I have hitherto been unable to reconcile the experiments to the theory. I have collated all your experiments which have appeared to me to be connected with your theory, and have arranged them for your perusal in as intelligible a manner as I can. I have commenced with the machine experiments, the accuracy of which I by no means question. I know that your facilities are great, and you have taken great pains in conducting them.

The articles from 296 to 303 furnish very interesting data respecting electro-dynamic action by modified discharges through the galvanometer. By article 296 I find that the fluid excited by forty turns of the machine was discharged from the whole battery of fifteen jars, through the galvanometer; and that by the influence of this discharge "the needle immediately moved," but to what extent is

not mentioned. It is stated, however, in article 297 that, "by repeating this action, (a discharge of fluid from forty turns of the machine,) a few times, the vibrations soon extended to forty degrees."

It would be impossible to form a just estimate of the number of discharges that was transmitted to produce the deflection of 40°, by merely reading the statement; but by making a few experiments with electric forces with different degrees of power, it is soon discovered that the first deflection, (if electro-magnetic) by a discharge from forty turns of the machine, was much less than 22°; or, five divisions and a half of the card; which is the amount of deflection due to *thirty turns* only, see articles 362 and 363. If the number of discharges were four only, the probability is, that the first deflection did not amount to 10° by the discharge of forty turns; (article 296.)

The experiments described in article 302 are exceedingly interesting, and perhaps, more in point than those already quoted. But before they can be properly understood, it is necessary to become acquainted with some preliminary matter, which very fortunately, is amply furnished by other passages in your paper. By article 301, I am informed that "the needle was so adjusted that, whilst vibrating in moderate arcs, it required time equal to twenty-five beats of a watch to pass in one direction through the arc, and of course, an equal time to pass in the other direction." I learn also, from article 369, that your watch beats "one hundred and fifty" times "in a minute." Therefore, the "twenty-five beats" required for one swing of the needle, amounts to 10″ of time.

Another piece of necessary information, is the speed of the machine; which I find very carefully described in article 290. "Each turn of the machine, when worked moderately, occupies about four fifths of a second." So that in ten seconds, or the time required for one swing of the needle, the machine would perform twelve and a half revolutions.

Thus prepared, I proceed to the experiments described in article 302. "The current, direct from the machine, was sent through the galvanometer for twenty-five beats, (a current from 12½ turns of the machine) then interrupted for other twenty-five beats, renewed for twenty-five beats, (12½ turns more,) again interrupted for an equal time, and so on continually," pouring in a discharge of 12½ turns each time, and taking every advantage of the *recondite* momentum of the needle by timing its occult motions. What was the result? Your answer is, "the needle soon began to vibrate visibly."

I think, Sir, you will allow, that three discharges of 12½ turns each, performed as stated in article 302, would transmit about the same quantity of fluid through the galvanometer as one discharge from the jars, (296;) allowing for the consequent loss whilst charging them; and that the whole charge was transmitted through the coil. But if I am to judge from the expression, "and so on continually," I am led to believe that there were more than three discharges or more electricity than that excited by 37½ turns of the machine, transmitted through the galvanometer, before the needle "began to vibrate visibly." Taking, however three discharges only, for the interpretation, of "and so on continually," then accordingly with the views you have taken, the deflections in these two cases ought to be equal, because the quantities were equal. Hence if the needle merely moved by the 37½ turns as described in article 302, it ought to have shown but a very trifling vibration by the discharge from the jars as described in article 296. If, however 10° be allowed for the deflection by the first discharge of forty turns of the machine, then, the arcs of deflection due to the quantity of fluid excited by *ten turns*, discharged under some of the various circumstances which you have described, will stand as below.—

Articles in which the experiments are described.	Number of turns of the machine.	Deflections of the Galvanometer needle.
296	10	2·5°
302	10	less than a visible vibration
363	10	7·5°

I now beg to solicit your attention to a few remarks which I find necessary to make regarding your equivalent Voltaic experiment.

The "Voltaic arrangement" intended to produce a deflection of the galvanometer needle, equal to that produced by thirty turns of the machine, (22°,) I find described in article 369; and you have stated in article 372, that this arrangement would maintain a steady deflection of 21° 20′ (5⅓ divisions of 4° each.) And again you have said, (in article 370) that this same "Voltaic arrangement" produced a transient deflection of 5½ divisions of the card, or 22° by closing the circuit for "eight beats of the watch" or 3⅟ seconds of time.

It is not any part of my business to enquire into the cause which led you to this extraordinary statement of the results of an experiment with the character of which I have

long been familiar; but I beg to be permitted toexplain a certain particular concerning it, in which, I am persuaded, you will feel much interest; because you are well aware that it is from one of considerable experience in matters of this kind. The information I have to communicate is simply this.—Any Voltaic arrangement such as you have described, and sufficiently active to maintain a *steady* deflection of 21° 20′ would, by suddenly closing the circuit, after the metals were immersed, and retaining it closed for 3½ seconds of time, drive the needle through an arc of 70° at least, or more than three times the arc of transient deflection which is described in article 370.

It may possibly be asserted that I have taken a different method of making the experiment to that described in your paper. Such an assertion would be perfectly correct; but I do not perceive that the experiment would have been any worse, or less satisfactory had it been conducted in that manner. It is very easy to discover that if the deflection of the needle in your experiment amounted to 22° only, the metals must have been dipped *very slowly* into the acid solution; and that the whole surfaces were *not* immersed 3½ seconds of time.

It appears to me also, that if the described results of this experiment had even been correct, the mode of conducting it was probably objectionable. It is named as an experiment with a "*voltaic* arrangement producing an effect equal to that described in article 363," (printed by mistake 367) by thirty turns of the machine.—see also article 376. But you are probably aware that the extent of transient deflections from sudden discharges of any given quantity of fluid will depend very much upon the initial velocity of transmission, or if you please, upon the initial intensity of the current. Now the intensity of a current from a jar is continually diminishing from the first to the last moment of transmission. Hence the initial intensity is the maximum. But from the manner in which your voltaic experiment was conducted, it appears very obvious that the initial intensity of the current was a minimum. Had the initial intensity been the maximum in the voltaic experiment, as it would have been had the metals been placed in the acid solution previously to closing the circuit, the result would have been very different to that stated in article 370. Indeed, it would be exceedingly difficult, by proceeding even in the same manner with the experiment as described in article 370, to *catch* a deflection of 22° only unless the needle were intentionally checked at that point.

Permit me now sir, to direct your attention to article 372 in which you say that "a permanent deflection to that

extent, (meaning 21° 20′) might be considered as indicating a constant voltaic current, which in eight beats of my watch, (369) could supply as much electricity as the electrical battery charged by thirty turns of the machine;" this inference obviously implies, that *permanent* and *transient* deflections give similar indications of an electric force under all circumstances; and that from the same source of excitation the extent of deflection, whether transient or permanent, ought to be the same, or nearly so; (compare articles 370 and 372,) which is contrary to all experience: for in many voltaic arrangements, it so happens that those which maintain the smallest angles of permanent deflections, give much the greatest angles of transient deflection; and in none that I have met with, is the transient angle less than double the permanent angle, when the time for electro-dynamic action exceeds one second; even though I employ needles much heavier than those you experimented with. The preceding article furnishes some information regarding transient and permanent deflections of the galvanometer needle by electro-dynamic action.

I am almost at a loss, sir, how to address you on the subject of your chemical experiments. They have excited my astonishment more than any of the rest. You however, "have ventured to discuss freely," the theories of the most illustrious electro-chemical philosophers that the world ever produced, "trusting that you should give no offence to their high minded authors;" "for," say you, "I felt convinced that if I were right, they would be pleased that their views should serve as stepping stones for the advance of science, and that if I were wrong, they would excuse the zeal that misled me, since it was exerted for the service of that great cause whose prosperity and progress they have desired." *

Trusting that these generous sentiments are expressive of your own liberality, I venture to proceed; and as you and I are both labouring in the same field of science, let another grand maxim be continually placed before us whilst in the pursuit of physical truths. "The first step towards acquiring knowledge is undoubtedly that which leads us to a discovery of the falsehoods of received opinions." I shall observe brevity as I proceed.

Thirty turns of the machine, says article 374, "made a dark brown spot penetrating to the second thickness of the paper, moistened at all times," says article 373 "to an equal degree in a standard solution of hydriodate of potassa." And article 375 says, that a "standard current

* Phil. Trans. for 1833, p. 683.

of voltaic electricity, continued for eight beats of the watch was equal in chemical effect to the thirty turns of the machine." I can perceive no facts that would sanction such a comparison; but, on the contrary, I have some reason to suspect that many are to be found which would militate decidedly against it. But granting for a moment that your comparison was a just one, I cannot see that these results had any connexion whatever withthe galvanometer experiments. The Voltaic arrangement for the one purpose was very different to that for the other; for, notwithstanding the same metals might be employed, the "drop" of "sulphuric acid to four ounces of distilled water," in one case, formed a very different arrangement to that in which the nitric acid was employed in the other.—Hence, whatever might be the chemical effectsof one of these arrangements, they can never be considered as indicative of the chemical powers of the other. Your opinion that " the heightened power of the Voltaic battery "was" a just "compensation for the bad conductor interposed," cannot be admitted into electrical science. There is no law to support it, nor any experiment in its favour. Your Voltaic arrangements were perfectly distinct, and your magnetic and chemical experiments had no bearing on one another. Had you employed *one* battery and *one* conducting circuit in both classes of Voltaic experiments, the results, whatever they might have been, would have commanded some degree of interest.

The voltaic battery in your chemical experiment was obviously very active, and the solution of hydriodate of potassa was pretty strong; otherwise a permanent deflection of 20° 21′ could *not* have been maintained. But then the solution of hydriodate being strong, tended more, perhaps, to effect its own decomposition than any other peculiarity in the experiment. With such an arrangement a brown stain on the test paper would be produced by the shortest possible contact, and instead of confining the decomposition to one solitary point, a dexterous experimenter might easily speckle some square inches of surface. If the face of your conductor, which laid on the test paper, had been the size of a sixpence, it would have stained to a deep brown the whole area of contact in less than eight beats of your watch.

You will now easily perceive my reason for not falling in with your views regarding the equality of the chemical powers of your machine and voltaic apparatus. I take for granted that your machine experiments are correct, and if so, it has not the one-hundredth part of the decomposing power of hydrodate of potash, as is exhibited by a voltaic apparatus such as you have described. With a similar

apparatus I have accomplished decompositions to the extent you have mentioned, with twelve plies of the test paper interposed: and I have done the same when the needle would not stand at 10°. The hydriodate is easily decomposed when the current will not sustain the needle at one degree of permanent deflection.

The solution of hydriodate of potassa which you employed, was obviously too strong for such investigations as you were engaged in. A brown speck of iodine would be presented on the paper by the slightest touch of the conductor; and the depth of the tint is a mere guess.—When the hydriodate is so far diluted as to require eight ten, or more seconds of time to liberate a visible spot of iodine, the action of voltaic pairs of various kinds, may very easily be compared. With such dilute solutions, chemical decomposition would become a tolerably good test of the relative actions of the machine and a voltaic pair; because the *time* of producing the first visible speck by each process, might then be easily measured.

Your position in article 377 is obviously as indefinite and as objectionable as that in articles 366 and 367. Or, if I may be permitted the expression, it is *doubly* objectionable: because it is both indefinite and implies that magnetic deflections and chemical decompositions go hand in hand, or are indicative of the extent of each other: Of this there is not a shadow of proof: you have shown no experiment in favour of it. It is, indeed, contrary to fact. I am not certain that you could have chosen an experiment less calculated to support your theoretical views than that with the hydriodate of potassa. Your theory mainly rests on this solitary experiment, which refuses to prove any thing for which it was selected.

I know well, sir, that you will be deeply interested in the contents of this letter, on which account, and also to prevent my meaning being misunderstood, I have been explicit on every point; and I hope that I have not been led into any unnecessary remark. I have admitted accuracy in all your machine experiments, because I have not had at command such powerful apparatus: but the errors you have fallen into in your Voltaic experiments are too palpable to escape notice, even of the humblest enquirer.—You may now, perhaps, be induced to re-examine, and, if necessary, correct the experiments I have alluded to.—Your explanation, or any observations you may be desirous of making regarding them, will find a welcome to the pages of these Annals.

I am Sir, with very great respect,
Your obedient Servant,
WILLIAM STURGEON.

*Relation by Measure of common and voltaic Electricity.**

361. Believing the point of identity to be satisfactorily established, I next endeavoured to obtain a common measure, or a known relation as to quantity, of the electricity excited by a machine, and that from a voltaic pile; for the purpose not only of confirming their indentity (378.) but also of demonstrating certain general principles (366, 377, &c.,) and creating an extension of the means of investigating and applying the chemical powers of this wonderful and subtile agent.

362. The first point to be determined was, whether the same absolute quantity of ordinary electricity, sent through a galvanometer under different circumstances, would cause the same deflection of the needle. An arbitrary scale was therefore attached to the galvanometer, each division of which was equal to about 4°, and the instrument arranged as in former experiments (296.) The machine (290,) battery (291.), and other parts of the apparatus were brought into good order, and retained for the time as nearly as possible in the same condition. The experiments were alternated so as to indicate any change in the condition of the apparatus, and supply the necessary corrections.

363. Seven of the battery jars were removed, and eight retained for present use. It was found that about forty turns would fully charge the eight jars. They were then charged by thirty turns of the machine, and discharged through the galvanometer, a thick wet string, about ten inches long, being included in the circuit. The needle was immediately deflected five divisions and a half, on the one side of the zero, and in vibrating passed as nearly as possible through five divisions and a half on the other side.

364. The other seven jars were then added to the eight, and the whole fifteen charged by thirty turns of the machine. The HENLEY's electrometer stood not quite half as high as before; but when the discharge was made through the galvanometer, previously at rest, the needle immediately vibrated, passing *exactly* to the same division as in the former instance. These experiments with eight and with fifteen jars were repeated several times alternately with the same results.

365. Other experiments were then made, in which all the battery was used, and its charge (being fifty turns of the machine,) sent through the galvanometer: but it was modified by being passed sometimes through a mere wet thread, sometimes through thirty-eight inches of thin string wetted by distilled water, and sometimes through a string

* From the transactions of the Royal Society, 1833.

of twelve times the thickness, only twelve inches in length, and soaked in dilute acid (298.). With the thick string the charge passed at once; with the thin string it occupied a sensible time, and with the thread it required two or three seconds before the electrometer fell entirely down. The current therefore must have varied extremely in intensity in these different cases, and yet the deflection of the needle was sensibly the same in all of them. If any difference occurred, it was that the thin string and thread caused greater deflection; and if there is any lateral transmission, as M. COLLADON says, through the silk in the galvanometer coil, it ought to have been so, because the intensity is lower and the lateral transmission less.

366. Hence it would appear that *if the same absolute quantity of electricity pass through the galvanometer, whatever may be its intensity, the deflecting force upon the magnetic needle is the same.*

367. The battery of fifteen jars was then charged by sixty revolutions of the machine, and discharged, as before through the galvanometer. The deflection of the needle was now as nearly as possible to the eleventh division, but the graduation was not accurate enough for me to assert that the arc was exactly double the former arc to the eye it appeared to be so. The probability is, that *the deflecting force of an electric current is directly proportional to the absolute quantity of electricity passed,* at whatever intensity that electricity may be.

368. Dr. RITCHIE has shown that in a case where the intensity of the electricity remained the same, the deflection of the magnetic needle was directly as the quantity of electricity passed through the galvanometer. Mr. HARRIS has shown that the heating power of common electricity on metallic wires is the same for the same quantity of electricity whatever its intensity might have previously been.

369. The next point was to obtain a *voltaic* arrangement producing an effect equal to that just described (367.). A platina and a zinc wire were passed through the same hole of a draw-plate, being then one eighteenth of an inch in diameter; these were fastened to a support, so that their lower ends projected, were parallel, and five sixteenths of an inch apart. The upper ends were well connected with the galvanometer wires. Some acid was diluted, and after various preliminary experiments, that adopted as a standard, which consisted of one drop strong sulphuric acid in four ounces distilled water. Finally, the time was noted which the needle required in swinging either from right to left or left to right; it was equal to seventeen beats of my watch, the latter giving one hundred and fifty

in a minute. The object of these preparations was to arrange a voltaic apparatus, which, by immersion in a given acid for a given time, much less than that required by the needle to swing in one direction, should give equal deflection to the instrument with the discharge of ordinary electricity from the battery (363. 364.); and a new part of the zinc wire having been brought into position with the platina, the comparative experiments were made.

370. On plunging the zinc and platina wires five eights of an inch deep into the acid, and retaining them there for eight beats of the watch (after which they were quickly withdrawn) the needle was deflected, and continued to advance in the same direction some time after the voltaic apparatus had been removed from the acid. It attained the five-and-a-half division, and then returned swinging an equal distance on the other side. This experiment was repeated many times, and always with the same result.

371. Hence, as an approximation, and judging from *magnetical force* only, at present (376.) it would appear that two wires, one of platina and one of zinc, each one eighteenth of an inch in diameter, placed five sixteenths of an inch apart, and immersed to the depth of five eighths of an inch in acid, consisting of one drop oil of vitriol and four ounces distilled water, at a temperature about 60°, and connected at the other extremities by a copper wire eighteen feet long and one eighteenth of an inch thick (being the wire of the galvanometer coils) yield as much electricity in eight beats of my watch, or in $\frac{8}{150}$ths of a minute, as the electrical battery charged by thirty turns of the large machine, in excellent order (363. 364) Notwithstanding this apparently enormous disproportion, the results are perfectly in harmony with those effects which are known to be produced by variations in the intensity and quantity of the electric fluid.

372. In order to procure a reference to *chemical action*, the wires were now retained immersed in the acid to the depth of five eighths of an inch, and the needle, when stationary, observed; it stood, as nearly as the unassisted eye could decide, at 5½ division. Hence a permanent deflection to that extent might be considered as indicating a constant voltaic current, which in eight beats of my watch (369.) could supply as much electricity as the electrical battery charged by thirty turns of the machine.

373. The following arrangements and results are selected from many that were made and obtained relative to chemical action. A platina wire one twelfth of an inch in diameter, weighing two hundred and sixty grains, had the extremity rendered plane, so as to offer a definite sur-

face equal to a circle of the same diameter as the wire; it was then connected in turn with the conductor of the machine, or with the voltaic apparatus (369.) so as always to form the positive pole, and at the same time retain a perpendicular position, that it might rest, with its whole weight, upon the test paper to be employed. The test paper itself was supported upon a platina spatula, connected either with the discharging train (292.) or with the negative wire of the voltaic apparatus, and it consisted of four thicknesses, moistened at all times to an equal degree in a standard solution of hydriodate of potassa (316.)

374. When the platina wire was connected with the prime conductor of the machine, and the spatula with the discharging train, ten turns of the machine had such decomposing power as to produce a pale round spot of iodine of the diameter of the wire; twenty turns made a much darker mark, and thirty turns made a dark brown spot penetrating to the second thickness of the paper.—The difference in effect produced by two or three turns, more or less, could be distinguished with facility.

375. The wire and spatula were then connected with the voltaic apparatus (369.) the galvanometer being also included in the arrangement; and a stronger acid having been prepared, consisting of nitric acid and water, the voltaic apparatus was immersed so far as to give a permanent deflection of the needle to the 5½ division (372.) the fourfold moistened paper intervening as before. Then by shifting the end of the wire from place to place upon the test paper the effect of the current for five, six, seven, or any number of the beats of the watch (369.) was observed, and compared with that of the machine. After alternating and repeating the experiments of comparison many times, it was constantly found that this standard current of voltaic electricity, continued for eight beats of the watch, was equal in chemical effect, to thirty turns of the machine; twenty-eight revolutions of the machine were sensibly too few.

376. Hence it results that both in *magnetic deflection* (371) and in *chemical force*, the current of electricity of the standard voltaic battery for eight beats of the watch was equal to that of the machine evolved by thirty revolutions.

377. It also follows that for this case of electro-chemical decomposition, and it is probable for all cases, that the *chemical power, like the magnetic force* (366.) *is in direct proportion to the absolute quantity of electricity* which passes.

378. Hence arises still further confirmation, if any were required, of the identity of common and voltaic electricity, and that the differences of intensity and quantity are quite sufficient to account for what were supposed to be their distinctive qualities.

379. The extension which the present investigations have enabled me to make of the facts and views constituting the theory of electro-chemical decomposition, will, with some other points of electrical doctrine, be almost immediately submitted to the Royal Society in another series of these researches.

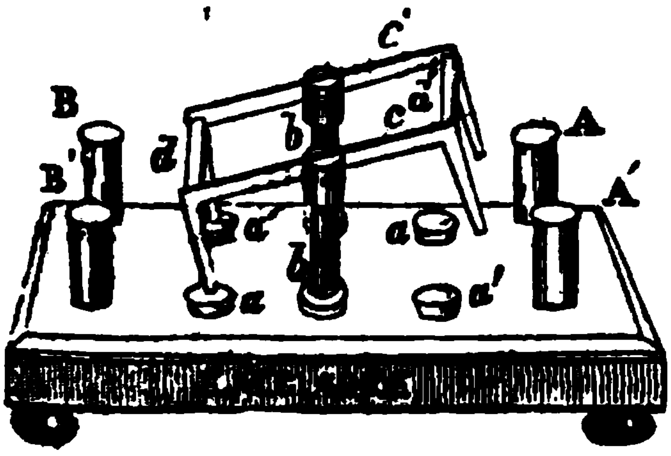
Royal Institution,
15*th Dec.* 1832.

X. *Description of* E. M. Clarke's *Electrepeter.*

To W. Sturgeon, Esq.

Dear Sir,

Understanding from you that descriptions of new Philosophical Instruments will find a ready insertion in your valuable work, I therefore send you a description of an instrument of my construction for changing the direction of electric currents, named by my worthy classical friend, Dr. Murphy, an *Electrepeter*. This instrument, you, sir, as a public lecturer, can fully appreciate; knowing the facility it affords of showing the changes that are produced when the direction of currents are reversed.



The most interesting application of this instrument is that when applied to an apparatus of your invention for showing the attraction and repulsion of Voltaic currents when induced in a mobile wire frame, timing the reversion of the electric currents, continuous rotary motion of the wire frame may be produced by the earth's magnetism.

A. A'. B. B'. four brass cups, screwing into and passing through the bottom board. *b. b'*. two brass pillars also screwing into and passing through the bottom board, having slits filed in their heads, into which two moveable brass frames *c. c'*. fit, being connected by the two ivory rods,

d. d'.; four brass studs *a. a. a'. a'.* screw into and pass through the bottom board, their upper surfaces being slightly concaved. The cups, studs, pillars and frames, are connected underneath the bottom board by pieces of copper wire soldered to them as follows;—

Cup A. and studs *a. a.*
Cup A'. and studs *a'. a'.*
Pillar *b.* and cup B.
Pillar *b'.* and cup B'.

Consequently whichever pole of the Voltaic, Magnetic, or Thermo-electric battery is in the cup A. the current passes on to the studs *a. a.* up the frame *c.* down the pillar *b.* on to the cup B. If you now reverse the position of the frames so as to bring their points in connection with the other two studs, then the direction of the same current will be from cup A, to stud *a.* up frame *c'*, down pillar *b'*, on to the cup B. It is only necessary to pour mercury into the four cups for the convenience of connecting the Electrepeter with the battery at one end, and the apparatus for the experiment at the other; it being immaterial which end you use.

It may be necessary to mention that when I first constructed this instrument I showed it to Dr. Faraday, who thought he had seen one like it described in some of Arago's papers: but on referring to his writings, he found that he had a contrivance for producing the same effect, but not so simple as mine. The Rev. T. W. M'Gauley exhibited, in part of his very ingenious Electro-magnetic experiments, an instrument to produce similar effects; but on referring to page 307 of the Philosophical Magazine for October, 1835, you will perceive mine is more universally applicable.

Believing that no person is better qualified nor none more deserving of success in your present undertaking,

I remain, sincerely,
Your obliged friend,
E. M. Clarke.
Magnetician.

No. 9, Agar St. West Strand,
London, Sept. 21, 1836.

XI. *On the Electric Shock from a single Pair of Voltaic Plates, by* Professor Henry, *of Yale College, United States*: *Repeated, and new Experiments, by* William Sturgeon, Lecturer, &c. &c.

A week or two before the Bristol meeting of the British Association, I was particularly gratified by an intimation which I had of an experiment made by Professor Dr. Henry of Yale College. My informant is a Mr. Peaboddy, a scientific American gentleman whom I accidentally met with in the Adelaide Gallery of Practical Science. The experiment as described to me, was to convert *quantity* of the electric fluid into *intensity*, by means of a single voltaic pair; the indication of intensity being that of producing a shock. Whether this be Dr. Henry's real meaning or not, I have no further means of ascertaining. It occurred to me at the time, from what I could learn from Mr. Peaboddy's description of the apparatus by which Dr. Henry had made the experiment, that the effect was due to the momentum of the fluid put into motion, not, perhaps, from its having a great degree of tension in the voltaic circuit, but from its being transferred suddenly to a new channel; in precisely the same way as shocks are produced by a magnetic electrical machine; which I explained to Mr. Peaboddy, and Mr. Clarke, Philosophical Instrument Maker, who was then in company with him. One of the principal circumstances to be attended to, in order to produce a shock, I observed, is that of having a sufficient extent of circuit: for whatever be the mode of excitation, the whole of the fluid belonging to the conducting wire will be put into motion: and if it moves with a sufficient celerity, the momentum it acquires will enable it to overcome the resistance of a worse conductor by suddenly transferring it from the former to the latter: and by this means, it might, perhaps, be transmitted through an inferior conductor, which, without such momentum, it could not penetrate.

I am not aware from what train of reasoning, Dr. Henry has been led to construct an instrument which will produce shocks by one pair of plates; but as I understand that the ingenious Professor has not yet published his invention, and as it is probable that Mr. Peaboddy may have told many other persons of the fact, I consider that I cannot render Dr. Henry a better service at this time, than by securing for him the credit of his experiment in the guardian pages of these Annals. I must observe, however, that as I am unable to describe the exact mode by which the experiment was made at Yale College, I cannot do any

more at present than describe that by which I have repeated it.

In fig. 16, A, B are two coils of copper wire, each containing about three hundred feet, and well covered with sewing silk. The inner ends of the wires forming the coils, are joined together by solder at S, and on the upper side of the joining is soldered a disc of copper, whose upper surface is quite bright. To the outer extremity of the wire belonging to B, is soldered a cylinder *n* of brass; and near to the outer extremity of that belonging to A, is soldered another brass handle P. The end *s*, of the wire S, Z, merely rests on the plate S, and the other end Z, is in connexion with the zinc side of the voltaic pair, and C, being connected with the copper, the apparatus is complete.

The coils which I have employed are some of those belonging to my magnetic electrical machines; and the battery one of my cylindric pots, holding about a quart of liquid.

Suppose now, a person with moistened hands, takes hold of the handles *n*, *p*, one in each hand; then the circuit would be made up of two channels; one very good conducting channel from the copper C, through the coil A, and round by S, to the zinc Z. The other from C to P, thence through the person connected, to the handle *n*; thence through the coil B, and along by S to Z. This latter channel is rendered a bad conductor because of the person being placed between *p* and *n*; and perhaps, by this intervention, the whole current travels by the former route in the direction of the arrows leading from C, and in the direction S, Z.—Now, it is obvious that the whole of the fluid belonging to the coil A, is kept in motion by the action of the battery; whilst that in B, is very little, if at all disturbed. Let now the end S of the wire S Z, be suddenly lifted off the plate: the fluid which is in motion in the coil A, can no longer travel towards Z because of the interruption at S. But as it has access to the coil B, it will, by its momentum, disturb all the fluid in that coil, and drive suddenly against that in the person situated in the circuit between the two handles *n*, *p*, who will in consequence experience a shock. Let now the position of the voltaic plates be changed, so that the current will flow in the reverse order, or from Z to C in the figure. In this case as well as in the former, the superior conducting circuit would be through the coil A, whilst the fluid in B. would remain nearly at rest. Again open the circuit at S. The fluid in motion in A, now rushing in the direction P, would drive against that in the person between *p* and *n*. As the fluid from the coil A pressed in this direction, it would be followed by that in the coil B,

which would facilitate the disturbing of that in the bad conductor, and conduce to the production of the shock.

If this explanation be admissible, it is easy to perceive that the shock would be produced in whichever direction the first current runs through the coil A. Although, perhaps, it might not appear so obvious, by what means the coil B contributes to the shock. I do not therefore give it with a view of supporting it, but as one which occurred to me at the time I was repeating Dr. Henry's experiment.—Figure 16 is very unlike the apparatus which Mr. Peaboddy described, but I believe it is the same in principle. Dr. Henry's apparatus consists of a long strip of sheet copper, (I was not told its length or breadth) formed into one coil in the manner of a watch spring, having one of its extremities in connexion with one of the plates of a large calorimoter, and about half way between the extremities of the copper strip is connected the other voltaic plate. And the person who experiences the shock is placed in connexion with the two extremities of the coil, in precisely the same manner as in fig. 16. I did not feel disposed, however, to cut two or three sheets of copper into strips for that purpose and having several coils of wire at hand, I considered that they, perhaps, might answer the purpose quite as well; and the arrangement I made was precisely that shown in fig. 16. A shock is produced every time the contact is broken at S, but none is given on completing it again.—This is just what happens with the magnetic electrical machine. If the upper side of the plate S be made rough with a file, a series of shocks is produced in rapid succession. By applying a small rough edged wheel to shake the end S of the wire Z S, in such a manner as to permit it to touch and untouch the plate, or break and make the contact in rapid alternation, the shocks are converted into a disagreeable pulsatory stream.

Having thus satisfied myself as far as my information of Dr. Henry's experiment had conducted me, I became desirous of ascertaining other particulars concerning the arrangement of the apparatus; but at that time I had no opportunity of carrying on my enquiries in the manner I wished, nor was it till the 23d of the present month that I could find time for that purpose.

My first object was to ascertain whether or no the coil B was conducive to the shock; and after many trials it was found to lessen, rather than increase the intensity.

In order to explain the manner by which I ascertained this fact, the reader must imagine the handle *n*, fig. 16, to be connected with the coil A at S, and the coil B taken away. The first principal current by this arrangement,

through the coil A from C to Z as decidedly as before, but when the connexion was broken at S, the current from A rushed immediately towards *n*, and consequently to the person placed between *n* and *p*. This variation of the experiment proves the coil B to be of no use in the manner it was before used; or indeed, something worse than useless, because the shock was more powerful without it.—Although I had tried both arrangements several times over, yet, as I was employing rather an active battery, I still suspected that I might possibly fall into error, in consequence of the difficulty of keeping such a battery in uniform action. This thought led me to try weaker acid solutions, and eventually I resorted to salt and water for the exciting liquid, and began with a new pair of metals. Still, however, I found that the coil A alone did better than with B attached.

It very often happens in experimental researches, that we are led to digress from the path of enquiry we have previously marked for the pursuit, by the appearance of some unexpected fact which obtrudes on our notice. And it was from discovering that salt water was a sufficient exciter to produce very smart shocks in this instance, that I was led completely from my principal line of pursuit, to enquire how far the experiment would permit the battery to be diminished in size. It will be of no interest to many of my readers concerning the route of my experiments on this point; I will tell them at once that I soon reduced it to the size of a lady's thimble; and still produced considerable shocks. Eventually I tried two wires, one copper, the other zinc, about one twelfth of an inch thick, and immersed to about an inch deep in diluted nitrous acid.—Even with this miniature battery smart shocks were produced: and I have no doubt that the smallest fragments of metal might be made to produce sensible shocks.

Having ascertained this curious fact, I returned to the original line of enquiry; and endeavoured to ascertain how far the length of the wire was concerned. The coils were now connected by solder at S, and the wire S Z, soldered to the outer end of the wire belonging to B, near to the lower arrow beside the handle *n*: and a contrivance for opening and closing the circuit was placed between *c* and the joining of that wire with the handle P, so that when the voltaic circuit was broken, there would still be a metallic connection from *p* to *n*.

With this arrangement the principal circuit would be from C to Z through both coils, six hundred feet of wire; and when the circuit was broken about the arrow at C, the fluid now in motion in both coils would produce a disturb-

ance of the fluid in the person joining *n, p,* and a shock would be experienced, which the experiment proves. The shock by this arrangement is not so great as by one coil only. The coils were next tried side by side, so as to form a double channel of three hundred feet. Very little was gained by this arrangement. The effect in some trials appeared rather greater, in others rather less than by one coil only.

Another coil was now tried. The wire was rather stouter than the former and the reel was of wood. The reels of the coils before used were of metal, which it was thought might possibly affect the results. The wire on the wooden reel was the same length as on either of the others, viz. three hundred feet. The shocks were equal, if not superior to those with one of the other coils.

Remarks. The shock is produced entirely without the voltaic circuit.

The spark is much brighter than when no coil is in the circuit.

The shock is never produced only at the moment of opening the voltaic circuit, none being experienced when the contact is made.

If there be any spark whilst making the contact, or closing the circuit, it is exceedingly feeble when compared to that seen when the circuit is opening.

About 300 feet of copper wire has, by these experiments, answered better than 600 feet in sequence.

With 50 feet no shock could be perceived.

A single wire 300 feet long has answered as well, or better than two such wires forming a double conductor.

Copper wire, one twentieth of an inch diameter answers better than thinner wire.

A pair of cylinders, one of copper, the other of zinc, which will enter a pint porcelain jar, and excited with cold salt water, is quite a sufficient voltaic power to produce very smart shocks.

With acid and water, and thin copper and zinc wires one inch long, shocks may be produced.

With regard to the length and thickness of the conducting wire, it is considered that much may possibly depend upon the extent and nature of excitement of the voltaic surfaces. No batteries were caused but such as have been described.

Artillery Place, WILLIAM STURGEON.
Sep. 24, 1836.

Postscript to the above.

Since the preceding paper was written and sent to press it occurred to me that I had neglected a certain point in

the enquiry, which might, perhaps, affect the results very materially. I thought that if the coils had had each a nucleus of soft iron in it, the magnetism which would be brought into play might possibly either increase or diminish the shock. Indeed I had no idea what might happen which was quite enough to begin with. A few experiments however, soon convinced me that the iron had not much influence, either one way or the other; and if my curiosity could have rested here, I might have saved myself a great deal of trouble by terminating my experiments at this point. I could not however get entirely rid of the magnetism which had thus entered my mind, and a train of ideas which had long ago been formed now flashed with redoubled force into my recollection.

It has been my opinion for many years, that in all cases of electro-magnetic action, where a needle or other ferruginous body is operated on, there is an intermediate agent; and that the deflections, &c. are not the immediate effects of the electric matter, but are secondary effects; the primary effects being the magnetizing, or polarizing of the magnetic matter in the conductors, and perhaps, of that also of the surrounding medium, It is on this principle that I have always in my lectures explained the attractions and repulsions of parallel conducting wires, phenomena first shown by the late M. Ampere; and I have stated this to be my opinion, in my paper on the theory of magnetic electricity published in the 81st vol. of the "Philosophical Magazine, &c." as will appear by the following quotation.

"It appears to me that electric currents generated by magnetic agency are not the *immediate* effects of the magnet employed in the excitation. It is highly probable that there is a *mediate* or intervening agent called forth; the magnetism natural to the excited metal, which, by being polarized by the exciting *polar magnetic lines* of the magnet becomes the immediate agent in giving life and energy to the previously dormant electricity of the metal.

"Remote and mysterious as the intermediate agency of the natural magnetism of the metal in this process of exciting electricity may appear in the present infantile stage of the science, I have much reason to suppose that such is the fact. The phenomena in magnetic electricity, as well as those in electro-magnetism, are highly favourable to this hypothesis; and I am not aware of an exception that militates directly against it. Moreover, the facility with which the *modus operandi* might be explained upon the simple principles of *polar magnetic lines* alone, would, I am persuaded, establish a degree of probability at least, not easily shaken by any counter-reasoning likely to be

advanced; and the illustrations which it would be possible to bring forward in support of such an hypothesis, might possibly be the means of fixing a basis on which the theory of excitation in this curious branch of physics is eventually and permanently to be established.

"The same class of remote laws apply epually to electro-magnetism as to magnetic electricity; and it would be very difficult indeed, independently of those laws, to completely harmonize with each other the phenomena displayed by the two different modes of excitation."

Although these views of electro-magnetic action have so long occupied my attention, I have never had sufficient spare time from my other pursuits, to arrange my ideas and explain facts by means of this hypothesis. But now, having the implements in my hands, and a point of determination regarding the correctness or incorrectness of my views appearing just within my reach, I determined to put the question immediately to the test of experiment. But I must explain a little farther by what reasoning I was induced to undertake a whole day's labour to satisfy myself on this one point; and I am very sorry the drawings are all gone to press, otherwise I might have drawn a figure that would have facilitated the explanation very much. Fig. 3 must answer as a substitute.

If the tangential lines in that figure be virtual magnets, they ought to assist each other in their development, when they are placed sufficiently near to each other. Imagine that C and Z, are the ends of a bent conducting wire, whose end is behind the paper. The magnetic force in every transverse section would then be represented by those tangential lines, or by lines similarly situated. Let the spectator now imagine that the end Z of the wire is continued to a sufficient length to turn upwards over the end C, and proceed to behind the paper, turn ronnd the former bend, and arrive again below the section *Z*. The wire would now form a coil of two convolutions.

Let, now, sections of this last convolution be exposed to view, one above C, the other below Z. The tangential lines in these sections, and of those in every succeeding convolution would observe the same arrangement as those in the figure. But the tangential lines on the *inner* side of the last convolution, will be in the reverse order to those on the *onter* side of the first, on which they are superposed. The north poles of one convolution will be opposite to the south poles of the other, between the wires in every section that can be imagined; and the same will happen between every two convolutions throughout the coil, whatever may be its extent. And if sections be drawn on both sides of

C and Z, it will be observed that the arrangement is precisely of the same character, and that south and north poles are presented to each other in every part of the coil.

Under these circumstances then, it would appear that when once the magnetic arrangement is accomplished in the coil wire, the polar attractions of the elementary magnets will tend in some degree, to keep them thus arranged; by which means that part of the electric force which is equivalent to the aggregate force of all the multitudes of magnetic attractions, will be relieved from that duty, and will join and assist the remaining electric force in overcoming other resistances which it has to contend with; for whatever be the nature of the resistance that an electric current has to overcome, it tends more or less to retard its motion. Magnetism is not brought into play in the conducting wire, nor in iron in its neighbourhood, without an expenditure of some part of the electric force; and consequently the electric force lost by keeping a straight wire in a state of magnetic polarity, would be in some ratio with the extent of magnetism displayed. But this would not be the case if the wire were formed into a close packed coil. The polar magnetic energies once developed, would be arranged in the best possible order for mutual polar attraction, and the magnetic resistance thereby very much abated. The coil wire would thus be rendered a better conductor, and would permit the current to flow more rapidly than if no such relief had been afforded; and consequently the momentum ought to be greater by the conductor being in a close coil than when straight or even loosely folded in a skein; and if the shocks depend upon the momentum, they ought to be *more intense* by the conductor being in a compact coil.

Such were the results of my reasoning on this point, and nothing less than an ardent hope of realizing them experimentally, could have induced me to undertake the labour which I then saw placed in the way to truth.

I had only fifty feet of silked wire that was uncoiled; this was placed in the voltaic circuit; but no shocks could be perceived.—The wire was next coiled closely round a cylinder of wood, and again placed in the circuit but still no shocks were discovered. I was not much surprised at these results, because I considered that the wire was probably too short. I therefore determined on uncoiling one of my 300 feet wires which had previously been ascertained to be conducive to the shock. I had three of these coils.

This was done, and the wire loosely hung round a chair back was placed in the circuit; no shock could be produced.

The wire again coiled on the reel, and again placed in the circuit. The shocks were as powerful as at first.

Another wire was uncoiled and placed in the circuit.— No shock could be produced. The wire again coiled.— The shock as great as at first.

The third wire was uncoiled, and placed in the circuit. No shock again.

The wire was again coiled and again placed in the circuit. The shock was as great as at first. These results were exceedingly gratifying to me, not so much at having satisfied my curiosity, as by the success which had attended my labours in bringing to light a novel principle, which, though long perceived by the mind, had in every other respect been permitted to remain in concealment; and, till this day, unregistered in the pages of philosophy.

The development of this physical truth which must now be enrolled in the service of science, unfolds a new and interesting field for philosophical contemplation. It throws much light on the *modus operandi* of every electro-magnetic phenomena, and must be a guide to our future reasoning on this interesting branch of science.

I am well aware that other facts may appear necessary to establish the correctness of the hypothesis, which led me to this discovery, and doubts may possibly arise respecting the increase of velocity of the current, upon the principles of reasoning which conducted me to that conclusion.— Time alone must decide these matters. I have not one moment left to give it another consideration. It is now eleven o'clock at night, and I have been at work almost without intermission since seven in the morning, and every other part of this number is now in the press. I will, therefore, content myself for the present, with registering the fact, leaving my hypothesis to the decision of others.

It just occurs to me whilst writing, and looking at the rough sketches which I have drawn to assist the arrangement of my ideas, that it is possible an impulse may be given to the current by the sudden transiliancy of the magnetic tangential lines from a state of vigorous polarity to that of complete annihilation; at the precise moment the battery action is cut off.

Sept 28th, 1836. W. S.

XII. *Description of an Electro-magnetic Engine for Turning Machinery, by* William Sturgeon, Lecturer, &c. &c.

In fig. 17, A, A, A, A, represents a stout square board, which forms the base of the engine. In two opposite cor-

ners of the base board, are fixed the two upright pillars B, B, which carry a cross piece C, C. In this cross piece are fixed two other smaller pillars P, P, which also carry a cross piece. In the centre of the engine is a vertical shaft, which turns freely in two metallic collars; one of which is in the centre of the base board, and the other in the centre of the cross piece C, C.

About half way up the shaft are two circular channels, one above the other as seen in the figure. Through the centre of these channels, and at right angles to their planes, the shaft passes, and is fixed to them. Lower down, the shaft passes through the centre of an opening in the cross piece *d*, *d*, also supported by two short pillars. On this cross piece, and concentric with the shaft, are fixed four quadrantal metallic plates, separated from each other by narrow radial openings.

Near the top of the shaft, and at right angles to it, is fixed a compound bar magnet N′ S′, each magnet of which is about eighteen inches long, one inch broad, and half an inch thick. Near to the bottom of the shaft is fixed another similar compound magnet N S, with its poles in the opposite direction to the former.

In a circle concentric with the shaft, and at an equal distance from each other, are fixed in the base board, the lower extremities of four cylindrical bars of soft iron, *i*, *i*, *i*, *i*, each of which is enclosed by six coils of copper wire. The coils round each cylinder are separated from each other by intervening cases of oil silk. Each set of extremities of these copper wires is soldered to one stout copper wire; hence the extremities of the twenty-four coils terminate in eight of these latter wires, four of which proceed from the lower extremities of the coils, and are soldered to the four quadrantal metallic plates, one to each. The other four stout wires proceed from the upper parts of the coils and terminate by proper connexions in the circular channels, which are partly filled with mercury. Through the sides of the channels pass four metallic stems, two through each, their inner extremities being in contact with the mercury in their respective channels. The stems of each pair are placed at 90° from each other, and the whole at right angles to the shaft. The right angle which the upper pair forms, is on the opposite side of the shaft, to that formed by the lower pair. From each stem hangs a metallic wire, reaching obliquely to its respective quadrantal plate on the cross piece *d*, *d*, which maintains a connexion between these plates and the mercury in the circular channels; transferring the electric current from one plate to another, and consequently from one coil to another, in their progress of revolution.

To prevent the figure being distorted, none of these connecting wires are drawn.

The engine is put in motion by the application of two cylindrical voltaic batteries of a single pair each, the metals being placed in two porcelain jars, each of which holds about three pints. These batteries are connected with the conductors of the engine at their terminal cups. One battery at each end of the lowest cross piece *d d*.

The connexions being properly made, the iron cylinders become magnetic in succession, and by the joint attractive and repulsive forces of the permanent magnets N′ S′, N S, and the temporary magnets *i, i, i, i,* the former, with the shaft and appendages, are pulled and driven round, the action being carried on in the following manner.

Imagine that the pole N′ of the permanent magnet is placed directly between the poles *s* and *n* of the temporary magnets; it will by this means be attracted by the former, and at the same time repelled by the latter. Hence it will be urged by both these forces towards the pole *s*. If now the contrivance be such, that the voltaic connexions be broken just before the pole N′ arrives at *s*, the extremity *s* of the iron bar will become neutral; but the momentum of the machine will carry the pole N′ to beyond this neutral point. Now conceive that the pendent wires have been carried from their last quadrantal plates to the next in succession. The currents by this means have been reversed in all the coils, and a corresponding inversion of polarity has taken place in the vertical iron bars; hence when the pole N′ has just passed the first bar, and whilst still in motion by its acquired momentum, it will again be urged on by two other forces, in a similar manner as by the two first. For the extremity having changed its polarity, it will now repel the pole N′ and drive it onward, whilst at the same time it will be attracted by the next bar in succession. And in consequence of similar changes of polarity taking place in all the four bars, the pole N′ is kept continually revolving.

All that has been said respecting the pole N′ applies equally to the opposite pole S′ of the same magnet. So that by this means, the magnet and its appendages are continually urged on by four forces, two attractions and two repulsions; and by considering that the lower magnet N S, is, by the contrivance also urged on at the same time and in a similar manner, by four other like forces, it will easily be understood that the two magnets, with the shaft to which they are attached, are kept in motion by eight forces; four of which are attractive and four repulsive. Such is the contrivance for keeping the machine in motion.

To the upper end of the shaft is attached a vertical spindle, carrying an endless screw near its upper extremity, and revolving as the shaft revolves, in a collar in the upper cross piece. Near to the lower end of this spindle is a fly with three arms, equidistant from each other, and each terminating with a heavy brass crescent. It was originally the fly of a roasting jack. The endless screw works in the teeth of a brass wheel, also a part of the old jack. The arbor of this wheel runs in a frame attached at right angles, to the upper cross piece.

This engine was constructed in the Autumn of 1832, and was exhibited for the first time in London, on the 21st of March, 1833, in a lecture on electro-magnetism, which I delivered at the Western Literary and Scientific Institution. And notwithstanding its then rude appearance, the Committee were so highly pleased with its structure and performance, that they expressed a wish to have it brought forward again, and hear it explained as soon as there was another opportunity. I was consequently honoured with an engagement to continue and extend my course of lectures in the following June; and in those lectures my engine again worked well, and excited a great deal of curiosity among the members of the Institution; and I believe was so fortunate as to give general satisfaction.

Since that time I have had attached to it contrivances for drawing water, waggons, and carriages on a railway, for sawing wood, pumping water, &c. upon about the same scale as we see pieces of machinery put into motion by the large models of steam engines. But as I saw several parts in which I thought it might be improved, it has long since been lain by, and another one is now building. The old one, however, is still in existence.

XIII. MISCELLANEOUS ARTICLES.

British Association.—Bristol Meeting. *Discoveries in Electricity by* Mr. Cross.

Mr. Cross related to three distinct subjects; atmospheric electricity, improvements in the galvanic machine, and the application of electricity to the production of crystals found in the mineral kingdom, but which can either not be obtained at all, or with great difficulty, in the laboratory of the chemist. His experiments upon the electricity of the sky were performed on the grandest scale, and with effect proportionally striking and brilliant, in consequence of his conductor being a wire of prodigious length, and being

insulated, and other wires arranged in a very skilful manner. The results however, at which he arrived concur with those of other philosophers. For clear and steady weather the air is always positive, but in cloudy and foggy weather is perpetually varying from positive to negative, sometimes also appearing to be in the neutral state. These phenomena, which would appear easily deducible from the principle of induction, as applied to the action of a cloud at different distances from the conductor, the intervening stratum of air varying in the degree of its conducting power, Mr. Cross explains by supposing an electrified cloud as an aggregate of discontinuous masses of vapour, or that it is composed of a succession of zones not in contact, and maintained by the inductive influence they exercise upon each other in oppositely excited states. Such also is, he finds the case of fogs; and he stated that upon one occasion he distinctly found a mist which drifted across his apparatus to be composed of, as he expressed it, a succession of *marching columns* in opposite electrical states. Upon the effects which he produced by operating with the electricity of the atmosphere, it is not necessary to dwell. By its means he ignited combustible bodies, tore in pieces the most rigid materials, resolved chemical compounds into their elements, magnetized, and in a word, reproduced, though upon a much more gigantic scale, all the known agencies of the artificial forms, of the electric influence. Mr. Cross's observations upon the pile need not detain us long. The improvements he suggests that the plates of the same couple should be brought as close as possible to each other, and immersed in glass jars, so that the different pairs should be, as much as possible in a state of insulation, have been suggested by others, the former by Drs. Hare and Faraday, and the latter by Professor Hare, who employed cylindrical glass vessels in his celebrated calorimeter. This latter contrivance, indeed, is very old, for it was employed by Volta in his Couronne des Tasses, one of the very first modifications of his extraordinary instrument, constructed by him. The views put forward by Mr. Cross, in reference to the origin of galvanic electricity, do not agree with those at present generally entertained. He abandons what is called the chemical theory, and reverts to the views originally propounded by Volta, namely, that the electricity is the result of the contact of the metals, and the acid merely acts as a conductor. We had nearly forgot too, to mention that Mr. Cross never found his electrometer affected by the Aurora Borealis, sheet lightning halos, or other luminous asmospheric phenomena. The latter part of Mr. Cross's address was decidedly the most valuable, but having been already detailed under the head of the proceedings at the Geological Section, it would

be improper, because superfluous, to return to the subject. We will however, observe, that the principle of his method is not at all new, the only difference between it and that of Becquerel being, as was pointed out by Dr. C. Henry, that the latter employed electricity of low tensions, while Mr. Cross operated with powerful batteries. The results of both are, of course, different, as they directed attention to different substances, but, with the exception of silicæ, which Mr. Cross conceives that he has crystallized, they carry with them an equal degree of interest.

Mr. Rainey, Surgeon, Maze Pond, Borough, having a fews days ago given me a polite invitation to look at his magnetic apparatus, I paid him a visit yesterday for that purpose, and was very much pleased with some of the instruments which that gentleman showed me. Mr. Rainey has a pretty little magnetic electrical machine, which gives very powerful and disagreeable shocks. It will also deflect the leaves of an electroscope, and charge a jar to a low intensity. The divergency of the electroscope was not new to me, but the charge of the jar was perfectly so. For I had not paid much attention to it myself, nor had I seen it done by others. I was however, more particularly interested with the ingenious manner in which Mr. Rainey had followed up my views respecting the charging of extensive surfaces of thin imperfect insulating substances* by his magnetic electrical machine. About three yards of varnished silk cloth were coated on both sides by broad strips of tin foil, and coiled round a cylindrical nucleus of wood; an intervening ply of silk preventing the two coatings from touching one another.

One of these coatings being connected with the positive and the other with the negative side of the machine, the silk became charged as decidedly as glass is charged by the common electric machine; but the intensity is very low. The spark is much finer by this means than from the coils alone.

Mr. Rainey also showed me, in a very satisfactory manner, his method of increasing the power of steel horse-shoe magnets by the application of soft iron alone. The magnetometer which Mr. Rainey has contrived, is really a neat and intelligent instrument. It shows the attractive and repulsive power of magnets at different distances; and is intended to be applied to the measurement of the intensity of terrestrial magnetism. This instrument will shortly be offered to the notice of the Royal Society.

September 14, 1836. WILLIAM STURGEON.

* See page 41 of these Annals, and Phil. Mag. for Aug. last.

THE ANNALS

OF

ELECTRICITY, MAGNETISM, AND CHEMISTRY;

AND

Guardian of Experimental Science.

JANUARY, 1837.

XIV. *Voltaic Batteries with amalgamated Zinc.*

The first account we have of amalgamated zinc being employed in Voltaic arrangements is in Sir Humphrey Davy's celebrated Bakerian Lecture for 1826;* in which it is simply stated, amongst a mass of other novel and exceedingly interesting facts which that eminent chemical philosopher then brought to light and advanced in support of the peculiar theoretical views he was discussing, that "zinc in amalgamation with mercury is positive with respect to pure zinc;"* without any allusion whatever as to the probable beneficial employment of it in the general construction of Voltaic batteries.

Mr. Kemp, of Edinburgh, we believe, was the first person who employed amalgamated zinc, and copper, in the regular construction of apparatus of this kind; which are clearly described in Professor Jameson's New Edinburgh Philosophical Journal, for December, 1828. The following extract is from Mr. Kemp's very interesting paper.

"The great expense of every form of Galvanic apparatus that has hitherto been offered to the scientific world has, without doubt, prevented, in some measure, the various interesting phenomena which Galvanism presents, the laws which govern its action, and its connexion with magnetism and electricity, from being more fully investigated.

"In performing many of the more important experiments in the science, different sets of batteries are necessary, according as the substances to be acted upon are more or less perfect

* Philosophical Transactions for 1826, part iii.

conductors: and the cost of these batteries being very considerable, few private individuals are possessed of an apparatus sufficiently varied for investigating the different branches of this subject.

"Independently, however, of the expense in procuring the requisite apparatus, another point is to be taken into consideration, and that of the utmost importance, viz.—the rapidity with which zinc plates become oxidated, rendering them completely useless for exciting the galvanic fluid, before the batteries are half worn out: and even in conducting experiments where much time is required for observing the results, the quantity of oxide formed on the surface of the zinc plates first retards the action, and finally prevents the motion of the fluid.

"A battery, therefore, which might be procured at little expense (the principal material being in the possession of every chemist), where little or no oxide is formed on the surface of the metal, and where the action is much more continuous, would be a very great desideratum, to supply which is the intention of the following paper.

"It had frequently occurred to me that mercury might be used as one of the metals for forming galvanic apparatus; and, from the difficulty with which it is acted upon by most of the acids, would answer the purpose of a negative metal better than any other, gold and platinum excepted; unless, indeed, its fluidity destroyed the capability it otherwise possessed for exciting galvanic energy.

"After several attempts to obviate this seeming difficulty, I at last succeeded in producing the following apparatus:— A B, C D, fig. 18. Plate III. represents a circular wooden cup, about half an inch deep, and about three inches in diameter, having a projecting rim A B. E F is a circular convex plate of zinc, attached to the cup, and about an eighth of an inch from it, by a wire either of copper or zinc, the extremity of which passes through, and projects within, the bottom of the cup, about the one-eighth of an inch: the whole is then rendered water-tight by a coating of wax, care being taken to keep the projecting point of the wire quite free from wax.

"A quantity of mercury, merely sufficient to cover the bottom, is poured into the cup, which is in contact with the zinc plate E F, through the medium of the wire. Over this is poured as much diluted muriatic acid as will nearly fill the cup. In this manner we have obtained one complete plate, consisting of zinc, mercury, and acid: and, by a continuation of the series, they may be increased to any extent.

"In erecting this pile, after the mercury and acid have been poured into the cups, as stated, they are to be placed above each other, as appears in fig. 21. Plate III.: the zinc plate of one will then be in contact with the acid contained in that immediately under it; and the cup itself resting on the small cheek cut round the lower part of the rim. In this manner, any number of plates may be arranged, and, if requisite, kept in their proper places by means of glass rods inserted into the base. This, however, is only necessary when the pile is of small diameter; for in one of a larger diameter a sufficient number may be raised, or they can be arranged in two or more columns, and connected as in the common pile.

"For the purpose of experimenting with this apparatus, a small brass socket, G, passes into the base, and communicates with the mercury in the undermost cup: into this socket a hole is drilled for inserting the wire. Another is attached to the capital or uppermost plate of the pile, from which a wire can be brought to complete the circuit, and varied according as the experiment requires.

"The zinc plate is made convex, in order to allow the hydrogen formed at its under surface to escape, which would otherwise collect into a globule, and displace the acid: the use of the projecting edge on the cups is to prevent the acid running over, and forming a communication between the different parts of the pile, and destroying the action.

"In this arrangement, as the zinc is the metal acted upon by the acid, it very soon becomes corroded, and, in this respect, is liable to the same objections as the ordinary galvanic apparatus. And in a battery where the negative metal is liquid, and the positive solid, no increase of power is obtained over the ordinary apparatus;—a circumstance which would seem to indicate that the negative liquid metal acts merely the part of a conductor: nor can it, while the positive remains solid, transmit the full effect of larger batteries, but must necessarily reduce it in the same proportion as a solid pile: the effect, however, would be very different, were the positive plate liquid, and the negative solid. This I have endeavoured to accomplish by the following arrangement, in which the positive plate is an amalgam of mercury and zinc.

"The form of it is the same as that of the pile already described, with this material difference, that instead of pure mercury, copper is used as the negative plate, and instead of zinc, an amalgam of zinc and mercury is the positive one: and whether we take into consideration the new field it opens for tracing the laws which govern galvanic action—its powerful effects on the magnet, and in the combustion of metals—

or the rapidity with which it decomposes imperfect conductors, this instrument must be acknowledged to be of some importance.

"A B C D, fig. 18. Plate III. represents a circular wooden cup half an inch in depth, and three in diameter, having a projecting rim A B. H is a small button of wood turned on the bottom of the cup at its centre, and which projects one-eighth of an inch from it. E F is a circular plate of copper attached to the cup, by means of a wire of the same metal, on which a screw is formed. The wire passes through the cup and screws into a brass nut L, fig. 20, which is sunk into the inside of it; the copper plate being kept at its proper distance by the button of wood. The whole is rendered tight by a coating of wax, care being taken to keep the nut and the projecting point of the wire free from the wax.

"The copper plate E F, fig. 19. is perforated with holes, to allow the hydrogen, as it is formed at the surface of the zinc and mercury, to pass up through it and escape, which would otherwise collect into a globule at its under surface, and force the diluted acid over the edge of the cups, and destroy the action of the pile. A plate of wire gauze, or a copper wire coiled round so as to form a plate, will answer the purpose equally well, as it allows the hydrogen to pass freely through the interstices, at the same time that it presents a favourable surface for conducting the fluid.

"A quantity of liquid amalgam of zinc and mercury, merely sufficient to cover the bottom, is to be poured into the cup, which will be in contact with the copper plate E F, through the medium of the nut and wire; over this is poured as much dilute muriatic acid as will nearly fill the cup. In this manner we have obtained one complete plate, consisting of copper, the amalgam of mercury and zinc, and the acid, and, by continuing the series, they may be increased to any extent.

"In this arrangement, the amalgam of zinc and mercury becomes the positive plate, while the copper is rendered negative.

"For the purpose of experimenting with this apparatus, the amalgam of mercury and zinc having been poured into the cups as stated, they are to be placed above each other as appears in fig. 21; the undermost plate of which becomes the negative pole, and the uppermost the positive. The circuit is formed the same as in the pile already described.

"The formation of the amalgam of mercury and zinc, which is used as the positive plate in this arrangement, is easily effected, and the preparation occupies only a few minutes. A quantity of zinc in fragments being put into a crucible, over

which is poured about four or five times its weight of mercury, and heat applied to it by a common fire, when the mercury arrives at its boiling point, the zinc will be found to be completely dissolved. To this composition, while warm, any quantity of mercury may be added, which will combine with it, in the same manner as if it had been heated along with it in the crucible.

"The amalgam, when once prepared, is fit for use as long as any of the zinc remains in the solution; and as the quantity dissolved at each time by the acid is very small, the same amalgam may be employed for a considerable time; and indeed so long as a particle of zinc remains in combination with the mercury. When the whole is dissolved, the mercury is left in a perfectly pure state, without any diminution having taken place, for the zinc alone is acted upon; and when this is the case, the former process has only to be repeated.

"The amalgam, when once made, may be kept for any length of time, in vessels excluding it from the action of the atmosphere, and may be poured from thence in one moment, when wanted, into cups of the requisite size.

"Without at present entering into detail regarding the laws which govern the action of this pile, farther than that they appear to be very different from any other, which seems to depend upon the positive plate being liquid, I shall merely notice the following experiments, which were performed by means of a pile, consisting of twelve cups one inch and a half in diameter.

"Having poured into each of them as much of the amalgam of mercury and zinc as covered the bottom, and added the muriatic acid and water, (the proportions being ten parts water, one of acid, and two of muriate of soda, these however may be varied), I placed them above each other, as shown in fig. 21; the copper plate of one being in contact with the acid of that immediately beneath it, throughout the whole series.

"I may here notice an important fact connected with this pile,—that in this state the cups may remain for any length of time ready for experimenting with, without almost any decrease of galvanic energy. This is certainly a decided superiority over any other form of galvanic apparatus, for it is well known, that it is only at the first instant after being charged, that batteries produce their full effect; at every succeeding instant it becomes less; and in a short time it entirely ceases. This seems to depend upon the particles of zinc, which having perfect freedom of motion in the mercury, are attracted by the copper plate with which they are in contact,

through the medium of the wire, and, by this means, the mercury is alone exposed to the acid, which has no action upon it. But upon the destruction of the electrical tension, by completing the circuit, the particles of zinc are no longer attracted by the copper plate, and having perfect freedom of motion in the mercury, rise to the surface, are acted upon by the acid, and have again a tendency to restore the pile to its former state of tension. It will thus be perceived, that the action going on in the pile, and, consequently, the quantity of electricity evolved, are each in exact proportion to the conducting power of the substance employed to complete the circuit; and this fact is still farther illustrated by the experiments.

"In the first experiment, I completed the circuit by two small platinum wires, and immersing them in a glass containing pure water, a slow action immediately took place in the body of the pile, and the water was decomposed, the hydrogen appearing at the negative, and the oxygen at the positive wire. I then added to the water a quantity of sulphate of soda, to incréase its conducting power. A more rapid action immediately took place in the pile, and a proportionately increased decomposition was effected in the solution.

"When a leaf of silver is employed to complete the circuit, by means of a wire and a plate of zinc, a continuous combustion takes place, and also a more rapid action goes on in the pile.

"The combustion of metals by this pile seems to be totally different from that produced by solid batteries; for the more the zinc plate is brought into contact with the leaf silver, the greater is the generation of electricity in the pile; and consequently the more rapid the combustion. In the solid pile, the quantity of electricity seems to be generated in a progressive manner; and it is only at the moment of contact that combustion takes place, (at least when the plates are small) some time being always necessary to recover its electric state.

"If a very perfect conductor, such as a metallic wire, be made to form the circuit, the generation of the electricity in the pile is astonishingly increased. These experiments all tend to prove that the action in the pile is exactly proportionate to the conducting power of the substances employed to form the circuit.

"The electro-magnetic phenomena which it presents are no less interesting. Having rendered a common sewing needle magnetic, and suspended it by a single fibre of silk, I brought the connecting wire over it, at the distance of an inch and a half, and the energy was of sufficient intensity to cause it to

stand at right angles to its natural position; nor was the effect much decreased, when, instead of twelve, I used only two of the cups.

"The same effect could not be produced by a battery of sixty plates four inches square.

"Another cause of the increase of power in this pile besides liquidity, is, that almost no oxide is formed on the surface of the amalgam of zinc and mercury, as is the case when a solid metal is used; for, as soon as the particles of zinc come in contact with the acid, they become oxidated; and as there is not the same cohesive attraction to hold them together as in solid metals, they are immediately dissolved and taken up by the acid, the amalgam always presenting to the action of the acid a surface perfectly clear, free from oxide, and in the most favourable circumstances for generating and transmitting the galvanic energies."

Mr. Kemp describes some other ingenious Voltaic arrangements in the same paper; some of which were all fluid; viz. two fluid metals and acid solutions; but as they are not so effective as that already described, it would be occupying room to no purpose, to detail the particulars of their construction. Mr. Kemp has employed a single cup as a battery for the exhibition of electro-magnetic phenomena; and as its description is short we copy it from the same volume of the New Edinburgh Philosophical Journal.

"The present construction of the apparatus consists of a circular wooden box, A B, fig. 22, Plate III, four inches in diameter and one deep. From the opposite sides of the box, project two small glass tubes C D, forming two cup-like cavities on the outside of it; only one of them C, opening into, and having a communication with, the inside of the box. About half an inch from the bottom, the diameter is lessened nearly a quarter of an inch, so as to form a projecting cheek round the inside of it, as seen in the plan of it, fig. 19. On this projecting cheek rests a circular plate of copper, perforated with holes, and having a wire of the same metal soldered to it.

"In order to generate galvanism by this apparatus, a quantity of the liquid amalgam of zinc and mercury is to be poured into it, as much as merely to cover the bottom, and rise into the small tube, which communicates with the inside of the box. The copper plate is then to be placed on the cheek, and over the whole is to be poured as much of a solution of muriatic acid and water as will fill the space between the amalgam and the copper plate, and rise a little above it. The extremity of the wire which is soldered to the copper plate is made to dip into the tube D, and has no communication with

the inside of the box, and into which is also poured a small portion of mercury.

"The apparatus being thus prepared, a communication is formed by wires passing from the two cup-like tubes. The one of these cups C, communicating with the amalgam, constitutes the positive pole; and the other D, being in connexion with the copper, by means of the wire which dips into it, forms the negative pole."

Mr. Kemp, after this description of his miniature battery, details several highly interesting experiments, which it is probable we may have to refer to on another occasion.

XV. Mr. Sturgeon's *Voltaic Experiments with Amalgamated Zinc.*

These experiments were published in the year 1830 in the "Experimental Researches on Electro-magnetism &c." mentioned in page 10 of these Annals. The following is an extract from the "Experimental Researches, &c." commencing at page 41.

"Let two equal slips of sheet zinc, of any convenient size, be polished with glass paper. Let the surface of one of them be amalgamated, by spreading mercury over it with a piece of clean rag, so that it may become quite brilliant, (or the zinc may be amalgamated by first dipping it in a solution of sulphuric acid, and afterwards in mercury). Both pieces being furnished with connecting wires, and in proper communication with the galvanometer, let them be plunged into a weak solution of either sulphuric or muriatic acid. The amalgamated piece will operate as zinc, and of course the other piece as copper, in a battery formed of these latter metals. But it will be observed, particularly if the combination be placed in a glass vessel, that the piece which operates as copper, undergoes *rapid destruction*, whilst the other is scarcely affected by chemical action. Gas will copiously ascend from the former, whilst a few indolent bubbles only will be observed on the latter, which cling to its surface without making their escape.

"Were it not on account of the brittleness and other inconveniences occasioned by the incorporation of the mercury with the zinc, amalgamation of the surfaces of zinc plates in galvanic batteries would become an important improvement; for the metal would last much longer, and remain bright for a considerable time, even for several successive hours—essential considerations in the employment of this apparatus.

"Notwithstanding the inconveniences, however, the improvement afforded by amalgamating the surfaces of zinc plates becomes available in many experiments; for the violent and intense chemical action which is exercised on zinc by a solution of sulphuric or muriatic acid, with the consequent evolution of heat and annoying liberation of hydrogen, have no place when the plates are amalgamated. The action is tranquil and uniform, and the disengagement of gas, which is trifling, occurs only when the circuit is complete, and at the surface of the copper plate only. The electric powers are highly exalted, and continue in play much longer than with pure zinc; and the only care of the experimenter is to prevent the copper, or whatever metal be substituted, from becoming amalgamated.

"With a solution of nitrous acid, the electrical energies of two pieces of zinc, the one pure, and the other amalgamated, are displayed in a very superior degree; but in consequence of the amalgamated surface becoming partially oxidized, and liberating gas, the experiment is not so decisively opposed to the chemical theory of galvanism, as when muriatic or sulphuric acid is employed.

"Two pieces of rolled zinc, each presenting ten square inches of surface, one of which was amalgamated and made quite brilliant, were formed into a Voltaic combination, with nitrous acid diluted with twelve times its quantity of water, and connected with the galvanometer. The needle, after several oscillations, reposed at an angle of 65°, after which, the following results were observed, without in the least disturbing the apparatus :—

	Minutes.		Degrees.
In	5	after the first immersion	70
—	20	- - - - - - - - - - - - - -	68
—	30	- - - - - - - - - - - - - -	65
—	45	- - - - - - - - - - - - - -	60
—	60	- - - - - - - - - - - - - -	55
—	75	- - - - - - - - - - - - - -	50
—	90	- - - - - - - - - - - - - -	45
—	105	- - - - - - - - - - - - - -	40
—	120	- - - - - - - - - - - - - -	35
In four hours		- - - - - - - - - - - -	25

"The metals were not disturbed for fifteen hours afterwards, at the end of which time the needle marked an angle of 18 degrees. An interruption was now made in the circuit, without disturbing the metallic plates; when the needle had re-

posed in the magnetic meridian the circuit was again completed, and the needle deflected to an angle of 30 degrees, and became steady at 19 degrees.

"The electrical powers displayed by two pieces of zinc, the one pure and the other amalgamated, and a solution of nitrous acid, are sufficiently energetic to produce electro-magnetic rotations, even on a pretty large scale. With similar pieces to those employed in the preceding experiment, the following amongst other electro-magnetic experiments may be exhibited.

"The apparatus in fig. 23, Plate III. represents an inverted horse-shoe magnet, which may be supplied with any kind of stand to keep it in a vertical position. A bent wire is supported on one of the magnetic poles by means of a fine point, on which it can move freely: whilst its depending extremities dip slightly into mercury which is placed in a circular wooden trough, through the centre of which is an opening for the admission of one of the branches of the magnet. The trough being adjusted to a proper height is to be kept firmly to the magnet, by means of a binding screw;—through the side of the trough passes a brass wire, the extremities of which are to be amalgamated for the purpose of preserving contact; one with the mercury contained in the trough and the other, by being bent upwards and passing through the bottom of a small cup, will communicate with another portion of mercury there placed. A cup, also containing mercury, is placed on the bend of the pendent wire, and there fixed by means of the upper part of the short wire, which forms the pivot passing through its bottom part, and that extremity of the pivot being amalgamated unites with the mercury in the cup.

"If now the plate of pure zinc be connected with the upper cup, as represented by the broken wire C, and the amalgamated plate with the lower cup by the broken wire Z, there will be a complete metallic communication between the two plates; and the circuit will be divided into two branches from the upper cup, down the two arms of the pendent wire to the mercury contained in the circular trough. The electric fluid will, therefore, by this arrangement, be transmitted through the pendent wire, in the direction indicated by the arrows; and by virtue of the electric and magnetic powers, the wire will revolve with great celerity round the magnetic pole. If the connecting wires be made to change places, the electric stream will be transmitted upwards in the two arms of the revolving wire, and the direction of motion will also change. If the apparatus be placed on the other magnetic pole, the wire will perform its revolutions in a reverse order with similar connexions."

The application of amalgamated zinc is again referred to at page 74, article 92, thus:—

"It has been observed that when zinc is amalgamated, its power of decomposing solutions of sulphuric acid becomes so far deteriorated, that the action is scarcely perceptible; whilst at the same time it forms a more energetic galvanic arrangement when combined with copper, or other metals, than if not so treated; zinc, therefore, in this state, becomes an exceedingly convenient metal for the exhibition of some of the most interesting experiments in galvanism: whether we regard the phenomena displayed to be contemplated by the philosopher, or for elegance of exhibition in the lecture room; and the *couronne des tasses* of Volta, and the *decomposing wires* of Sylvester, are amongst those which may be exhibited in the most satisfactory and elegant style.

"The *couronne des tasses* consists of a series of small glasses or cups, partly filled with an acid solution, and are generally arranged in a circle. Slips of copper and zinc are soldered end to end, and so arranged in the glass vessels, that the *copper* extremity of the *first*, is in the same glass with the zinc extremity of the *second*; the copper of the second with the zinc of the third; and so on in regular sequence: so that each glass in the circuit will receive a piece of *copper* and also a piece of *zinc*, which have no farther connexion with each other than by the fluid medium; the metallic communication being from one glass to another as in fig. 24, plate IV. If one of the compound metallic arcs be removed, as in the figure, the circuit becomes interrupted, and the extreme glasses become the poles of this miniature Voltaic battery. Gas will now be liberated at the surfaces of the zinc parts only; an effect which would be produced whether the copper were present or not. When the circuit is completed, by introducing the other compound arc, gas will also be liberated at the surface of every piece of copper in the arrangement. A solution of muriatic acid is usually employed in this experiment, but in consequence of that acid being sometimes of a yellowish tinge, and also acting chemically* on the zinc, the beauty of the experiment becomes partly obscured. When the zinc is amalgamated, and a limpid solution of sulphuric acid is employed, no such obscuration takes place: neither will any chemical action whatever be observed, at any other time than when the circuit is complete, and even then at the copper

* Electro-chemically, upon the principles illustrated in our article on the "electro-chemical action of metals on fluids" commencing at page 11, No. I.

surfaces only; showing, in an exceedingly beautiful manner, the influence of electric currents in the development of chemical action, which, by this mode of experimenting, can be created or annihilated with the velocity of lightning at the pleasure of the experimenter."

The respect which in this case was due to Mr. Kemp's labours, was acknowledged by the following note at the bottom of page 76.

"The reader will find in Jameson's Edinburgh Philosophical Journal, for October 1828, a description of some very ingenious applications of a soft amalgam of zinc, in the construction of galvanic batteries, by Mr. Kemp."

XVI. *Dr. Faraday's Experiments.*

Dr. Faraday, whilst speaking of the advantage of amalgamated zinc in Voltaic combinations, has very politely acknowledged the above experiments of Mr. Kemp and Mr. Sturgeon, and verifies their observations in the following words:

"I have had a small battery of ten pair of plates thus constructed, and am convinced that arrangements of this kind will be very important, especially in the development and illustration of the philosophical principles of this instrument. The metals I have used are amalgamated zinc and platina, connected together by being soldered to platina wires, the whole apparatus having the form of the couronne des tasses. The liquid used was dilute sulphuric acid of sp. gr. 1·25. No action took place upon the metals except when the electrodes* were in communication, and then the action upon the zinc was only in proportion to the decomposition in the experimental cell; for when the current was retarded there, it was retarded also in the battery, and no waste of the powers of the metal was incurred.

"In consequence of this circumstance, the acid in the cells remained active for a much longer time than usual. In fact, time did not tend to lower it in any sensible degree; for whilst the metal was preserved to be acted upon at the proper mo-

* "Electrodes" with Dr. Faraday, are what the ablest Electricians have hitherto called "poles." The curiosity of our readers respecting the origin of several modern *terms*, in this branch of science, will be amply satisfied by a perusal of our next number. Edit.

ment, the acid also was preserved almost at its first strength. Hence a constancy of action far beyond what can be obtained with the use of common zinc.

"Another excellent consequence was the renewal, during the interval of rest, between two experiments of the first and most efficient state. When an amalgamated zinc and a platinum plate, immersed in dilute sulphuric acid, are first connected, the current is very powerful, but instantly sinks very much in force, and, in some cases, actually falls to only an eighth or a tenth of that first produced.* This is due to the acid which is in contact with the zinc becoming neutralized by the oxide formed; the continued quick oxidation of the metal being thus prevented. With ordinary zinc, the evolution of gas at its surface tends to mingle all the liquid together, and thus bring fresh acid against the metal, by which the oxide formed there can be removed. With the amalgamated zinc battery, at every cessation of the current, the saline

* The rapid depression of action here spoken of by Dr. Faraday, is sadly at variance with the result of the following experiment.

A platinum and an amalgamated zinc plate were placed in a gallipot with dilute sulphuric acid, and properly connected with a galvanometer. Each plate exposed about four square inches of surface to the acid solution. The first steady deflection of the needle, marked an angle of 62·5°

In	5	minutes	60°	In	35	minutes	47
..	10	....	57	..	40	....	46·5
..	15	....	55	..	45	....	46
..	20	....	52·5	..	50	....	45
..	25	....	49	..	55	....	45
..	30	....	48	..	60	....	45

The metals were unmolested the whole of the time, and no opening made in the circuit from beginning to end of the experiment.

At the end of the hour that the metals had been in action, the contact was broken for one minute, and the acid solution agitated by shaking the metals in it. The connexions were again made, and when the needle ceased vibrating it reposed at an angle of 47°. The vibrations occupied one minute prior to the needle becoming steady.

The circle was kept closed for one hour more, at the end of which time the needle marked an angle of 42°. The needle had been nearly steady for the last half-hour, and showed no rapid decrease of power. At the end of the third hour, the angle of deflection was still 40°.

A considerable liberation of hydrogen at the platinum surface was observed all the time, certainly to the greatest extent during the first quarter of an hour.—Edit.

solution against the zinc is gradually diffused amongst the rest of the liquid: and upon the renewal of the contact with the electrodes, the zinc plates are found most favourably circumstanced for the production of a ready and powerful current."—*Phil. Trans. for* 1834, *p.* 458.

XVII. *Extract from a letter addressed to* MICHAEL FARADAY, *D. C. L., F. R. S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acad. of Science, Paris, Petersburgh, &c. By* J. FREDERIC DANIELL, *F. R. S. Prof. Chem., in King's College, London.**

Fig. 25 of Plate IV. represents a section of one of the cells, one of which is shown in perspective at fig. 26; *a b c d* is a cylinder of copper, six inches high and three and a half inches wide: it is open at the top *a b*, but closed at the bottom, except a collar *e f* one and a half inch wide, intended for the reception of a cork, into which a glass syphon tube *g h i j k* is fitted. On the top *a b*, a copper collar, corresponding with the one at the bottom, rests by two horizontal arms. Previously to the fixing the cork syphon tube in its place, a membranous tube formed of part of the gullet of an ox, is drawn through the lower collar *e f* and fastened with twine to the upper *l, m, n, o*, and when tightly fixed by the cork below forming an internal cavity to the cell communicating with the syphon tube in such a way as that when filled with any liquid to the level *m o*, any addition causes it to flow out at the aperture *k*. In this state, for any number of drops allowed to fall into the top of the cavity an equal number are discharged from the bottom *d* at the top of the zinc rod, various connexions of the copper and zinc of the different cells may be made by means of wires proceeding from one to the other.

In fig.† the ten cells are represented as connected in single series, the zinc of one with the copper of the next. They stand upon a small table in a circle, with the apertures of the syphon tubes turned inwards surrounding a large funnel communicating with the basin underneath for the reception of any liquid which may overflow. A smaller funnel is supported over the internal cavity of each cell by a ring sliding upon rods of brass, placed between each pair of cells. One of these

* Philosophical Transactions, for 1836.

† We have not printed the figure here alluded to, because Mr. Daniell subsequently dispensed with the funnels, and arranged his cylinders in another manner as will be seen in the next article. EDIT.

only is shown in the drawing to avoid the crowding of the sketch.

In the construction of this battery, I have availed myself of the power of reducing the surface of the generating plates to a minimum, the effective surface of one of the amalgamated zinc rods being less than ten square inches, whilst the internal surface of the copper cylinder to which it is opposed is nearly 72 inches. My principal objects have been to remove out of the circuit the oxide of zinc, which has been proved to be so injurious to the action of the common battery, as fast as the solution is formed, and to absorb the hydrogen evolved upon the copper without the precipitation of any substance which might deteriorate the latter.

The first is completely effected by the suspension of the rod in the interior membranous cell, into which the fresh acidulated water is allowed slowly to drop from a funnel suspended over it, and the aperture of which is adjusted for the purpose; whilst the heavier solution of the oxide is withdrawn from the bottom at an equal rate by the syphon tube. When both the exterior and interior cavities of the cell were charged with the same diluted acid, and connexion made between the zinc and the copper by means of a fine platinum wire $\frac{1}{100}$th of an inch in diameter, I found that the wire became red hot, and that the wet membrane presented no obstruction to the passage of the current.

The second object is attained by charging the exterior space surrounding the membrane with a saturated solution of sulphate of copper instead of diluted acid; upon completing the circuit the current passed freely through this solution; no hydrogen made its appearance upon the conducting plate, but a beautiful pink coating of pure copper was precipitated upon it, and thus perpetually renewed its surface.

When the whole battery was properly arranged and charged in this manner, no evolution of gas took place from the generating or conducting plates, either before or after the connexions were complete; but when a voltameter* was included in the circuit, its action was found to be very energetic. It was also much more steady and permanent than that of the ordinary battery; but still there was a gradual, but very slow, decline, which I traced at length to the weakening of the saline solution by the precipitation of the copper, and the consequent decline of its conducting power.

* An instrument for collecting the gases during the decomposition of water. *Electro-Gasometer* would be a more suitable name for such an instrument, because it merely measures the gases which are disengaged without indicating the amount, either absolute or relative of the electric fluid transmitted. See our next number. EDIT.

To obviate this defect, I suspended some solid sulphate of copper in small muslin bags which just dipped below the surface of the solution in the cylinders: which gradually dissolving as the precipitation proceeded, kept it in a state of saturation. This expedient fully answered the purpose, and I found the current perfectly steady for six hours together. This arrangement I have since improved by placing the salt in a perforated colander of copper fixed to the upper collar.

Fig. 27 represents a section of this additional arrangement. The colander with its central collar rests by a small ledge upon the rim of the cylinder. The membrane is drawn through the collar, and turning over its edge is fastened with twine.

After this alteration, the effective length of the zinc rods exposed to the action of the acid was found to be no more than four inches and a quarter. In ascertaining the powers of the battery in single series, the voltameter was the same that I have already described, the specific gravity of the solution of sulphate of copper was found to be 1198·5, and I commenced my experiments with the standard sulphuric acid, specific gravity 1027·3.

With this charge, after the circuit had been completed for ten minutes, the mean quantity of mixed gases taken at intervals of five minutes for two hours was 2·1 cubic inches, the results never varying more than 0·1 cubic inch from one another. The battery was then left in connexion, without the voltameter, for two hours and again examined for three intervals of five minutes, when the mean quantity of gases was found as before. It was again left in connexion for two hours, and re-examined, with the same result.

Upon adding nitric acid to the solution of sulphate of copper, I found that an injurious effect was produced; and that the mean quantity of gas in five minutes was lowered to 1·1 cubic inch; at this rate of action, the battery, however, remained steady for six hours.

Returning to the original solution of sulphate of copper for the exterior cavity of the cells, I doubled the strength of the dilute acid for the interior; adding for this purpose, 4½ measures of oil of vitriol, to 100 measures of water, sp. gr. 1056·2 and found the mean quantity of gas evolved in the voltameter increased to 3·8 cubic inches per five minutes. I then removed the meter and connected the circuit, leaving the battery in action for four hours. Upon re-placing the meter, it was ascertained to be still working at the same rate. The addition of nitric acid to the solution of the sulphate again reduced the rate to 2·1 cubic inches.

I now made trial of an addition of nitric acid to the interior

liquid of the cell, adding an equal volume to that of the sulphuric acid, and restoring the solution of the neutral sulphate to the exterior division. At the first immersion, an increased effect seemed to be produced, and the action for the first quarter of an hour was as high as 4·2 cubic inches per five minutes; but it ultimately settled down, and remained at the former amount without the nitric acid, of 3·8 cubic inches.

Here I may remark, that at the first immersion of the rods the effect of the battery is always a little higher than it afterwards settles at: and this I found to depend upon the evolution of a small quantity of hydrogen from a residue of local action upon the zinc, which adhered to its surface, and slightly impeded the action of the acid. These bubbles scarcely ever rose to the surface, and after the first quarter of an hour the battery took up a steady rate of action. By gently agitating the rods in the acid, the action would momentarily return to its original amount, but always settled down to the lower rate.

How very small this local action is, will be seen from the details of the following series of experiments, which I shall give at length. Upon this occasion I weighed the ten zinc rods, which were the same which I had used from the first; and were reduced to nearly half their original circumference, that I might judge how nearly the zinc oxidated would be equivalent to the volume of gases evolved: their weight was 2¼ lbs. and 13 grains (15·763 grains). I trebled the strength of the acid, adding 6¾ measures of oil of vitriol to 100 measures of water. sp. gr. 1079·4.* I sometimes made use of a large voltameter, the plates of which were 3 inches by 1 inch, and which having a bent tube, admitted of my receiving the gases into a large graduated jar, and sometimes of the same small meter, which I had hitherto employed. The results are contained in the following table.

* It may possibly be interesting to some of our readers to inform them, that Professor Daniell's method of expressing specific gravities, requires that the specific gravity of pure distilled water be considered as 1000. Edit.

Time.		Quantity of gas.	Rate per quarter of an hour,	Rate per five minutes.
Large Voltameter.				
h.	m.	Cubic inches.	Cubic inches.	Cubic inches.
11	24			
11	39	16	16	5·3
11	45			
11	60	30	14	4·6
12	3			
12	18	44	14	4·6
12	19			
12	34	58	14	4·6
12	49	71	13	4·3
Small Voltameter.				
12	51			
12	56	- - -	- - -	4·6
12	58			
1	3	- - -	- - -	4·6
Hydrogen removed by agitating the Rods.				
1	6			
1	11	- - -	- - -	4·9
Large Voltameter.				
1	14			
1	29	85	14	4·6
1	31			
2	1	27	13·5	4·5
2	31	54	13·5	4·5
3	1	82	14	4·6
Small Voltameter.				
3	2			
3	7	- - -	- - -	4·8

Thus the total quantity of mixed gases collected amounted to 186 cubic inches, which, being corrected for pressure, were equal to 188·48. The zinc rods were removed from the cells, rinsed in water, and carefully dried, when they were found to have lost 933 grains.

Now adopting the data for computation as laid down in your tenth series (1126), of 100 cubic inches of the mixed gases being equivalent to 12·68 grains of water, and taking the equivalent number of water as 9, and that of zinc as 32·5, we have the following proportions:—

Mixed gases. Cub. inch.		Water. grs.		Cub. inch.		grs.
100	:	12·68	: :	188	:	23·84

and

Equiv. water.		Equiv. zinc.		grs.		grs.
9	:	32·5	: :	23·84	:	86·1

showing the quantity of zinc equivalent to 188 cubic inches of the mixed gases to be 86·1 grains; differing from the quantity actually consumed in each cell only 7·2 grains, or 2·3 for each equivalent. The waste of metal, indeed, must have been even smaller than this; for at the bottom of the cells, I found a small quantity of amalgam which had fallen off the rods, and which, at the time, it was not convenient to collect, but which, if added to the weight, would have materially diminished the deficiency.

For this quantity of zinc 166·5 grains of metallic copper must have been precipitated on each copper cylinder; for not a bubble of hydrogen made its appearance upon them, and 6623 grains of crystalized sulphate of copper must have been consumed in the whole battery, or only 377 grains short of a pound avoirdupois.

Upon examining the cells after the course of experiments, the fresh precipitated copper had a most beautiful appearance, being of a bright pink colour. It was not only deposited upon the sides and bottom of the cylinders, but upon the under surface also of the colanders. In the angle formed by the junction of the bottoms with the sides and at the contact of the membrane with the collars of the colanders, it was collected in the largest quantities, and had a very distinct mamillated structure. At these points it is probable that the diffusion of the hydrogen had been impeded, and it had consequently accumulated in greatest abundance.

The charge of sulphate of copper has been left for weeks together in the battery, only taking care to keep the bottom of the colanders covered with the solid salt; but the acidulated water was withdrawn from the tubes every morning by means of a syphon, and a fresh charge substituted. It was generally found very slightly tinged with the sulphate, which would have been injurious to the zinc rods.

The only disadvantage that I am aware of in this new construction of the battery, is the unavoidable distance of the

generating from the conducting metal; which is five or six times greater than in the double Wollaston plates; and this I sought to obviate as much as possible by improving the conducting power of the interposed electrolyte. I made a trial of sulphuric acid of four times the standard strength, or 9 volumes of oil of vitriol to 100 of water sp. gr. 1105·4; and this I worked for six hours continuously, and found the result very steady: 5·0 cubic inches with the large voltameter, and 5·5 by the smaller, per five minutes of time.

Taking into consideration the great precipitation of the sulphate of copper, I am doubtful whether the solution could be kept in a state of saturation at much beyond this rate of work; and in cases where it may be desirable to maintain a constant action for a great length of time, it may not be desirable to carry it even so far; but this, with many other questions, I must leave for future examination.

Being now desirous to bring the *constant battery* into more immediate comparison with one of the usual construction, I took a trough with ten Wollaston plates four inches square, which had been but little used, and carefully cleaned, and fitted a double case of copper to the last zinc, to make the series complete. I charged it with a mixture in the proportion of 100 water, 2¼ oil of vitriol, and 2 nitric acid, and set it to work with the small voltameter. The following Table contains the results measured at intervals of five minutes.

Time.	Quantity of gas.	Rate per 5 min.
h. m.	cubic in.	cubic in.
10 22		
10 27	2·5	2·5
10 32	4·6	2·1
10 34	Voltameter refilled	
10 39	2.6	2·6
10 44	4·4	1·8
10 46	Voltameter refilled	
10 51	2·8	2·8
10 56	4·9	2·1
10 57	Voltameter refilled	
11 2	2·4	2·4
11 7	4·2	1·8
11 8	Voltameter refilled.	
11 13	1·8	1·8
11 18	3·2	1·4
11 19	Voltameter refilled	
11 24	1·3	1·3
11 29	2·2	0·9
11 34	3·0	0·8
11 39	3·55	0·55
11 44	4·1	0·55
11 49	4·5	0·4
11 54	4·9	0·4
11 59	5·25	0·35
12 0	Voltameter refilled	
12 15	0·9	0·3
12 30	1·7	0·26
12 45	2·25	0·26
1 0	2·75	0·25
	Battery left in connexion	
1 58		
2 13	0·1	0·03

This series of observations displays in a striking manner the peculiar irregularities of the common voltaic battery, and entirely agrees with those which I had previously made

with the dissected battery; the energy of the first contact declining even in the first five minutes a fourth or a third; recovering itself again by one minute's rest, and again declining; even with five such intervals of interrupted action, falling off permanently one half in the first hour: in the next hour of unceasing action rapidly falling to one tenth; and after four hours' connexion almost entirely ceasing.

If we compare the surfaces of the generating and conducting plates in each cell of the two batteries, we shall find that of the zinc in the Wollaston battery 32 square inches, whilst in the constant battery it does not exceed $7\frac{1}{2}$ inches. If we calculate only the interior surface of the copper of the former that is also 32 inches; but if both surfaces be efficient, (as I believe, though not to the same amount,) then it amounts to 64 inches. It can only, of course, be the interior surface of the copper cylinders of the *constant battery* which assists the action; and reckoning $5\frac{1}{2}$ inches in depth to be efficient, it amounts to $65\frac{1}{2}$ inches: to which if we add a small quantity for the bottom and underside of the colander, the amount of surface but little exceeds the former: but then it is all disposed to the greatest advantage. Under these circumstances the power of the constant battery is double that of the common battery at its first impulse, and can be maintained for any length of time in an invariable condition.

I shall trouble you, on the present occasion, with but one comparison more: and that is, of the efficiency of the two batteries under a retarding force. The charge which I used for the *constant battery* was only the double acid (sp. gr. 1050): but nevertheless I found that, with the opposition of three voltameters, the amount of gases in each was 0·9 cubic inch per five minutes: whilst a fresh set of Wollaston plates, with a new charge in the same proportions as before, gave only 0·32 cubic inch in the first five minutes; so that the superiority of the former, under these circumstances, was even greater than before.

I will only add, that I have kept 6 inches of platinum wire $\frac{1}{100}$ of an inch diameter, permanently red hot for a considerable length of time when the battery was merely connected in single series, and that the spark between charcoal points is remarkably beautiful; and shall reserve for a future communication, if you will permit it, some observations upon the different modes of combining the plates, for which it affords great facilities, and the different important applications to which, from the unvariableness of its action, it is applicable.

My principal object in these researches has been the attainment of this constancy; but, in addition, this new combina-

tion will be found, I think, to possess advantages which will secure to it a more general application than I at first contemplated.

First, the abolition of all local action by the facility of applying amalgamated zinc;

Second, the trifling expense of replacing the zinc rods when worn out (for they are easily cast and fitted in the laboratory); and the total absence of any wear of the copper;

Third, the non-necessity of employing nitric acid, and the substitution of the cheaper materials, sulphate of copper and oil of vitriol; to which I may add, the absence of any annoying fumes;

And, fourth, the facility and perfection with which all metallic communications may be made, and different combinations of the plates arranged.

Hoping that I may not have wearied you by these details,

I remain, dear Faraday,
Your very faithful friend,
J. F. DANIELL.

King's College,
January 23, 1836.

XVIII. *Additional Observations on Voltaic Combinations: in a Letter addressed to* MICHAEL FARADAY, *D. C. L. F. R. S. Fullerian Prof. Chem. Royal Institution. Corr. Memb. Royal and Imp. Acad. of Science, Paris, Petersburgh, &c. by* J. FREDERIC DANIELL, *F. R. S. Prof. Chem. in King's College, London.*

Received April 14th, Read April 21st, 1836.

My dear Faraday,

The Council of the Royal Society having done me the honour to order the publication of my observations upon "Voltaic combinations" in the Philosophical Transactions, I should wish to add the results of some further researches, which may render the account of the *constant voltaic battery* more complete and practically useful.

My great object in this combination was to obtain an invariable current of force sufficient to effect chemical decompositions, after overcoming the resistance necessary to register its quantity by the voltameter; and having succeeded in this, it seemed to me almost a matter of indifference to the solution of the various important questions to which it might be applied, whether the quantity were large or small. I

quickly, however, discovered that the battery might be rendered not only perfectly steady in action but very powerful; and that it would be extremely efficient and convenient for all the purposes to which the common voltaic battery is usually applied. I set myself therefore to perfect its construction with this view.

Before I state the results, I wish to direct your attention to some observations which affect the construction and application of your voltameter, being convinced that any thing which may tend to facilitate the use or establish the correctness of that most important instrument will prove of substantial benefit to electro-chemical science. In my previous communication, I stated that I had found that the plates of a voltameter only six eighths of an inch wide evolved the same amount of gases as two platinum plates of one inch wide, and of the same height, in one of the cells of the dissected battery, when the former was substituted in the circuit for the latter; and I imagined that "their nearer approximation had counterbalanced their deficiency of surface." The results of the experiments recorded in the Table (p. 98.) also show that there was no difference in the indications of the small meter and those of a larger, the plates of which were three inches by one inch when they were alternately used. The plates of the larger instrument were moveable, and admitted of adjustment to different distances from each other; and by these means I ascertained that no alteration within the distance of the generating and conducting plates of the battery, produced any difference in the results.

This rendered the non-influence of variation of surface still more remarkable: and wishing to push the observation to the utmost, I reduced the plates of a voltameter to the width of one eighth of an inch, retaining the same height of three inches, and still found its efficacy unimpaired. I next thrust two platinum wires one tenth of an inch in diameter through a cork closing the mouth of a glass tube, exposing about two inches of each to the dilute acid without any diminution of effect, the battery generating in all these experiments at the rate of 2·7 cubic inches per five minutes.

When I even covered the wires with a resinous cement, so as to have only one fourth of an inch of each exposed, the gases evolved in the same time amounted to 2·3 cubic inches. I ultimately coated the wires entirely with cement; and carefully bared their mere points with a file, when they still gave off the gases at the rate of 0·8 cubic inch per five minutes; and in this case the currents of the gases, instead of rising at once from the points of the horizontal wires, seemed to be

projected forward into the liquid with some force. This independence of the results upon the metallic surfaces of the voltameter is curiously contrasted with the paramount influence of the surface of the conducting plates of the cells of the battery, but it is probably owing to the absence of any active chemical affinity assisting or retarding the main current which circulates.

In the prosecution of my experiments I now began to perceive that it was by no means necessary to attend so closely to the supply of fresh acid to the battery as at first seemed advisable; and I ascertained that after an uninterrupted action of five hours, without its renewal, the voltameter only indicated a decline from 2·7 to 2·4. It being of great consequence to note to what extent the constancy of the action might be independent of the exact adjustment of the acid, I left the battery in connexion for twenty-four hours, at the expiration of which time 0·9 cubic inch of the mixed gases was collected in the voltameter in a quarter of an hour, or 0·3 cubic inch per five minutes. The acid was found to be almost perfectly saturated; a very small drop of dilute ammonia occasioning an instantaneous precipitation of oxide of zinc in the solution, which had a specific gravity of 1·276. In this state three quarters of a fluid ounce of fresh dilute acid was poured upon the top of the solution in each cell (the total charge of each cell being about 5½ fluid ounces) when the action not only rose to its original amount of 2·7 cubic inches, but to 4·2 cubic inches, at which rate of work it kept perfectly steady without any further renewal of the acid for four hours.

This increase of action I could only refer to the superior conducting power of the solution of sulphate of zinc; and two important points were thus indicated: 1st, that the conducting power of the electrolyte in the battery might be increased with great advantage; and 2dly, that the quantity of the circulating force was still more independent of the surface of the generating metal than my experiments had yet shown it to be. The length of the zinc rods exposed to the action of the fresh acid, which must have floated upon the saline solution in this experiment, could not much have exceeded one inch, and yet they proved perfectly efficient. I immediately confirmed this conclusion by shortening the zinc rods to one fourth of their original length, and found no diminution of the power of the battery when charged with fresh acid.

I next charged the battery with acid of the same strength as that which I employed in the voltameters; viz. eight parts of water to one part of oil of vitriol, by measure, (specific

gravity 1136), and obtained at once a steady action of 11 cubic inches per five minutes; this was therefore the mixture which I employed in all my subsequent experiments.

This rate of action must obviously require some attention to the state of the solutions in the battery, when perfect steadiness is required to be maintained for a long period; as nearly 25 grains of oxide of zinc are formed, and 154 grains of sulphate of copper decomposed in each cell per hour. Nevertheless it will remain constant for an hour and a half, even without any change of acid; and the addition every now and then of one fluid ounce of fresh acid will maintain the action for any desired time, provided the colanders be kept well supplied with the salt of copper.

Considering the great advantage which had been derived from the extension of the surface of the conducting metal of the battery, I now wished to ascertain whether this might be carried further; and for this purpose I caused some cells to be fitted up in the interior with ten small plates, one inch wide, extending from the bottom to the colander, and converging from the interior circumference towards the centre. In this way they just reached to the membranous tube, and the surface of the copper was more than trebled. I thought it possible that advantage might be thus derived, not only from the great extension of surface, but from the approximation of a part of the conducting plate to the generating rod.

My first experiment was made with five plain cells and five ribbed, working into separate voltameters, and I found the products equal, inclining, if any thing, in favour of the former. The amount of gases from each was, however, only 5·5 cubic inches per five minutes, or one half of that from the whole series of ten cells. When the ribbed and the plain cells were connected together in one series, the amount in one meter was, as before, 11 cubic inches.

It now seemed evident that a series of five did not confer *intensity* enough upon the current to enable its whole quantity to overcome the resistance of the conducting fluid between the two metals at the existing distance; and this being the case with the plain cells no increase of quantity could be expected to manifest itself from those with extended surfaces. When the latter were joined with the former, the smaller surface of course, governed the whole series, according to the established law. I therefore completed the number of the ribbed cells to ten, and setting them to work against an equal number of plain, the product of the latter was, as before, 11 cubic inches, but that of the former did not exceed 8·5. I have not yet been able to satisfy myself, whether this unex-

pected difference, in the opposite direction to what I expected, is dependent upon the construction of the cells, or upon some accidental circumstance which I have not been able to trace; but the result may certainly be taken to prove that no advantage arises from the extension or approximation of the conducting metal which I have described.

The increase of the number of the battery series requires, for convenience, a different arrangement from that I described in my last communication; and I now place the cells in two parallel lines of ten each, upon a long table, the syphon-tubes arranged opposite to each other, and hanging over a small gutter placed between the rows, to carry off the refuse solution when it is necessary to change the acid: and as the uniformity of action may be completely maintained by the occasional addition of a small quantity of fresh liquid, I have been able to dispense with the cumbrous addition of the dripping funnels. This arrangement admits with facility of any combination of the plates which may be desired.

I proceeded now to connect the cells together in pairs; the zinc rod of each ribbed cell being in communication with that of a plain cell, and the copper with the copper. The ten pairs were then connected in a series of ten: the product of this combination was 17 cubic inches per five minutes, or exactly double that of the single ribbed cells.

Considered in a theoretical point of view, these experiments seem to me to lead to the conclusion that the most perfect voltaic combination would consist of a solid sphere of a generating metal, surrounded by a hollow sphere of a conducting metal, with a stratum of intervening electrolyte perpetually renewed, and the metals communicating by a wire defended from the electrolyte by a glass tube covering that position which it would be necessary should pass through it in such a hypothetical arrangement, the resistance of the electrolyte would increase directly as the distances of the two spheres, or as the thickness of the stratum; while, supposing this resistance overcome, the quantity of force set in circulation would increase as the square of that distance from the centre, or as the surface of the exterior sphere. The number of a series required to give the necessary impulse would consequently increase only as the simple distance, while the advantage would increase as the square.

The rod of zinc within the cylinder of copper is probably the nearest practical approximation which can be made to such an arrangement; but the soundness of this deduction might doubtless be tested by varying the diameters of the cylinders.

The battery which I have now described, consisting of twenty cells, will, I think, be found amply sufficient for all the purposes of demonstration and investigation. It is competent to keep eight inches of platinum wire $\frac{1}{100}$ inch permanently red hot in the open air: and the amount of work which it is able to perform renders it even an economical source of the purest oxygen for laboratory purposes.

To facilitate this application, I have fitted up a cell by inclosing a platinum plate, instead of the zinc rod, within the membranous tube which is closed at the upper end by a glass tube bent in a convenient form to deliver the disengaged gas under a receiver. When this cell is included in the circuit of double cells, the hydrogen is absorbed as before, by the oxide of copper; but the oxygen is evolved at the rate of 84 cubic inches per hour.

I shall conclude these observations with the result of an experiment which places the secondary action of the affinities of the disengaged gases in a striking point of view. The mixed gases collected in a voltameter in five minutes were found to amount to 17 cubic inches, the oxygen collected in an equal time, when the hydrogen was absorbed, was 7 cubic inches; which are equivalent to 21 cubic inches of the mixed gases; and the hydrogen collected when the oxygen was absorbed (as is readily effected by reversing the connexions of the cell which I have just described) amount to 16 cubic inches, equivalent to about 24 cubic inches of the mixed gases.

Thus the removal of the hydrogen from the sphere of attraction allowed at an increase of action equal to 4 cubic inches, while the increase from the like removal of the oxygen was very nearly double; a difference which is probably referable to the equivalent combining volumes of the two gases. This observation opens a new field of enquiry of great interest, upon which, with your permission, I hope to have the pleasure of addressing you at no very distant period.

I remain, &c.

J. F. DANIELL.

King's College,
April 6, 1836.

XIX. *Description of a Voltaic battery, invented by* Frederic W. Mullins, Esq. *M. P., F. S. S. &c.*

A vertical section and perspective view of this battery are represented by figs. 28, and 29, Plate IV. C, C, C, fig. 28, is a copper cylindrical vessel inclosed at the bottom but open at top; C C, is another cylinder of copper, soldered to the bottom

of the former vessel, and concentric with its vertical sides. Round the top of the outer cylinder is a projecting shoulder, grooved like the sheave of a pulley for the reception of the open end of a bladder and string, the former of which envelops the whole cylinder, as shown by the waved line in the figure, and is kept in its place by several turns and a knot of the latter, over its open end in the groove. The inner copper cylinder is two, the outer about three and a quarter inches in diameter. The height of each six inches. The shoulder C C, projects outwards about $\frac{1}{10}$ of an inch.

Outside of the bladder is a cylinder Z Z, of stout rolled zinc, open at both ends, $3\frac{3}{4}$ inches in diameter, and $2\frac{1}{2}$ high. The whole are arranged as in fig. 28, in a white porcelain jar as seen in fig. 29. The connexions are made by narrow strips of sheet copper, which bend over the edge of the jar and enter two cylindric cups Z, C, containing mercury, as in fig. 29. These cups are fixed into a circular wooden base on which the battery stands. This battery is brought into action by two liquids: a solution of muriate of ammonia, which is placed outside the bladder; and dilute sulphuric acid charged with crystals of sulphate of copper, which is placed in the annular space between the two copper cylinders.

We have not as yet been favoured with Mr. Mullins's own account of his battery, but we have much pleasure in being enabled to give a just description of it, as made by Mr. Clarke; and of informing our readers that a battery of this kind, with Mr. Mullins's name on it, has been exhibiting in the Adelaide Gallery for the last six or more months, and that it works well.

It being our intention to make a series of experiments with a view of ascertaining the relative merits of the batteries here described: our readers may expect to have a just account of the results of those experiments as soon as completed. Should any improvements have been made, to any of these batteries, by their respective inventors further than here described, we shall have much pleasure in publishing them in our next number, provided the accounts of them arrive in time. Editor.

XX. *On an Optical Illusion observed during the action of Professor Ritchie's horizontal artificial voltaic magnet. By* Charles Tomlinson, Esq.

To the Editor of the Annals of Electricity, &c.

Sir,

As an ardent admirer and humble cultivator of electrical science, permit me to present you my best thanks for the

journal you have undertaken. Should the following facts be worthy of attention, they are at your service.

It is known that a rapidly moving wheel, or a revolving disk, on which any object is painted, seems perfectly stationary when illuminated by the explosion of the electric jar.

This beautiful fact, which is perfectly available to the scientific lecturer, for it can be seen and appreciated by a large audience, was first adduced by Professor Wheatstone, to show that the duration of the electric light embraces a point of time so extremely minute, that the revolving wheel or disk has not time to pass through any perceptible space during the electrical discharge, and therefore the disk or wheel appears stationary.

In two papers published in the third and fourth volumes of Dr. Thomson's *Records of General Science*, I have considerably extended this principle, and have by very various means produced the stationary appearance of a rapidly revolving disk, with many interesting modifications.

In a lecture recently given by me on electro-magnetism, I noticed a pleasing instance of the electric spark producing the stationary appearance. Professor Ritchie's horizontal rotating magnet is of course well known, and Mr. Clarke's valuable improvement of it, is, or ought to be, also well known. This latter apparatus, with a sustaining battery, will continue in rapid action for many hours, and produce very brilliant successions of sparks whenever contact is broken, and this with my apparatus occurs six or eight times per second; but, it often happens, that the mercury is thrown into large waves by the energetic action of the apparatus, in which case contact is broken, not at the line of division which separates the mercury into two equal portions, but at those points where the wire just emerges from the summits of the mercurial waves, and this is generally 90° from the line of division. One elevation or wave, too, often presents multiplied reflections of the sparks produced at other waves, or at one or other end of the line of division, and if the mercury be clean a very brilliant appearance is presented.

Now if all this be observed in a darkened room, the magnet, although performing as many as 400 revolutions per minute, will appear stationary, and its position will depend upon the situation of the spark or sparks at the time of observation. If the apparatus be very active, the waves will be constantly forming and breaking; then, the magnet's apparent stationary position will be as constantly varying, and the effect is extraordinary:—the magnet often appears broken into two or more parts, placed at various angles in respect of each other,

depending entirely upon the relative positions of the sparks. Suppose fig. 30 Plate V. to represent the mercurial flood-cup, divided into two portions by the line *a*, *b*, it is quite clear that, when contact is broken, a spark will appear at *b*, at one side and at *a*, on the other side, the magnet revolving in the direction of the arrows. Now, if by agitation, due to the rapid passage of the wires over the mercurial surface,* the mercury be depressed at *b*, and elevated into a wave at *c*, the wire from near *c* to *b*, is not in contact with the mercury, the spark will therefore be transferred from *b* to *c*; and if the same circumstance does not occur at *d*, that portion of the cup will be dark, and the magnet, illuminated in very rapid alternations by the two sparks *c* and *a*, will be presented to the observer as in fig. 31, Plate V.; the other half of the magnet not being illuminated by any spark, is of course not seen, unless the two sparks already mentioned, afford sufficient light to show the whole magnet.

But it sometimes happens that four sparks are seen at *a*, *b*, *c*, and *d*, which from the rapidity of the motion appear to be simultaneous, although of course they are not, then the other two halves of the magnet being added to fig. 31, will correctly represent the result.

When the magnet rotates steadily and only two sparks, *a*

* The mercury in the mercurial flood-cup is acted upon in three distinct ways which I may here notice.

1st. A mechanical action by which a portion of the mercury is scattered in globules over the upper portions of the cup, over the magnet, &c., and even over the table on which the apparatus stands to a distance of two or three feet. All this is effected by the wires dashing through the mercury, and the globules, which momentarily attach to the magnet itself, being scattered to the distance I speak of by the centrifugal force of the rotating magnet.

2d. By the continued action of the sparks minute portions of the mercury are fused, pass off in vapour, and are condensed on the under surface of the magnet in the form of globules so excessively minute that they lose their metallic lustre and appear dull white:—a lens, however, soon detects their form.

3d. A third portion of the mercury is converted into a black powder (formerly called the protoxide) and covers the surface of the mercury in the cup, and is formed evidently by agitation long continued in contact with the atmosphere. It consists probably of minutely divided metal and impurity, but as I have still obtained it when every precaution has been adopted to have pure mercury, its source is probably the copper wires dipping into the metal, or the metallic powder from the brass cup produced by the friction of the steel point on which the magnet revolves.

C. T.

and *b*, are seen, then the whole magnet appears stationary, and the illusion is most complete.

The cause of these phenomena is evidently the same as when a rapidly rotating disk appears stationary during an electric discharge; with the rapidly rotating magnet we have quick successions of sparks, each spark in this case producing the effect of the electric discharge in the former case; for, however quickly the sparks may succeed each other, each spark is separate and distinct from preceding and succeeding sparks, and the duration of any one spark embraces a point of time so extremely minute, that during such minute point of time, the magnet does not pass through any perceptible space and therefore it appears stationary.

The right angular appearance of the magnet, one half of which fig. 31 represents, is a good illustration of the extreme rapidity with which the sparks succeed each other; for this appearance depends upon the strictness of the fact that one leg of the right angle is formed after the other leg in point of time, because they depend for their appearance upon different sparks. Thus the sparks *a b* fig. 30 present the magnet and the line of division superposed, and the sparks *c* and *d* present the magnet 90° on either side of the line of division, but from the extreme rapidity with which the magnet moves and with which each pair of sparks alternates, the eye is cheated into a belief that the four sparks are simultaneous.

If the rotating magnet be viewed in the presence of other light, then the illusion which I have attempted to describe no longer exists, the magnet presenting a complete circle.

I have presumed fairly, I think, that most of your readers are acquainted with the whole apparatus, therefore I have not described it. Such of your readers as desire further information I refer to Professor Ritchie's paper in the Philosophical Magazine, or to Mr. Clarke, who seems to possess the valuable faculty of simplifying and rendering easy complicated and uncertain pieces of apparatus.

I shall shortly take an opportunity of calling the attention of electro-chemists to two points, which I propose to discuss in a future paper when my experiments are completed.

1st. On the action of direct solar light, of the separate colours of the solar spectrum, of diffused day light, and of artificial light upon electro-magnetism.

2d. On the influence of heat over electro-magnetic action.

I have to apologize for writing so much upon so trifling a subject, and remain, Sir,

Your obedient servant,

CHARLES TOMLINSON.

Salisbury, 18th October, 1836.

XXI. *Description of* Dr. Ritchie's *Rotating Magnet, mentioned in the last article.*

In fig. 32, Plate V., S N may represent the extreme portions of the branches of a horse-shoe magnet, kept steady in a verticle position by a heavy wooden, or metallic sole. Between these branches rises a pillar P, which passes through the centre of an annular wooden cell *a b*, (a plan of which is seen in fig. 30.) and terminates in a fine steel point, supporting the piece *n s*. The cell is divided into two compartments *c d*, as seen in the plan fig. 30, by two low partitions *a o, o b*, which do not rise so high as the edges of the vessel. The piece *n s*, is a small bar of soft iron, usually from four to six inches long, and about the diameter of a common curtain rod. It may be either cylindrical or a four sided, or any other prism. A hole with a fine bottom is drilled in the centre of the under side of this iron bar, for the reception of the steel point on which it is mounted and on which it rotates.* The bar is covered with several coils of silked copper wire, (one only is drawn in the fig.) the extremities of which are finely pointed, and bent downwards so as to reach into the cell *a b* fig. 32, but not so low as to touch the partitions *a o, o b*, fig. 30.

When an experiment is to be made with this apparatus, the compartments *c* and *d*, fig. 30, of the annular cell are to be partly filled with mercury, the surface of which is to receive the depending points of the wire, and to be kept in two distinct masses by the partitions. The boundaries of these mercurial surfaces curve downwards to the wood on every side, leaving the general level something higher than the wooden partitions, so that the points of the wire, (whilst the iron bar, *n s* fig. 32, is rotating in a horizontal plane, on the steel pivot,) will move in the mercury, till they arrive close at the partitions: the points there quit their respective metallic connexions, pass over the partitions, and enter, each that portion of mercury which the other had left.

The wires from a single pair of voltaic plates are to be connected with the mercury in the compartments *c* and *d* fig. 30, one with each. The electric current from the copper to the zinc will traverse the spiral which surrounds the iron bar *n s*, fig. 32, and convert the latter into an electro magnet,

* By this mode of suspension the rotating iron is apt to rock or wave about a good deal, to prevent which it is better to screw a piece of steel wire into the lower side of the bar for the pivot, which, being finely pointed, will run in a deep pivot-hole on the top of the pillar P.

which will immediately be operated on by the permanent steel horse-shoe magnet, S N, in the following manner.

Imagine the iron bar *n s*, fig. 32, to be placed at right angles to the plane of the permanent magnet N S, and the extremity *n* nearest to a spectator looking at the figure. With this position of *n s*, the point of the wire which bends down from *n* would be immersed in the nearest portion of mercury, and thatbending down from *s*, in the farthest portion. The horizontal positions of these portions of mercury would respectively correspond with those of the compartments *d* and *c* of fig. 30. The battery connexions being such that the extremities *n* and *s*, of the iron being respectively north and south polar they would be attracted by the permanent poles S and N. and at the same time respectively repelled by the poles N and S. By these four forces the extremity *n* would be urged towards S, and the extremity *s*, towards N, and the direction of motion would be that indicated by the arrows in fig. 30. Just before the axis of the rotating iron came to the plane of the permanent magnet the depending points would quit the mercury, the electric current would thus be cut off, and the electro-magnetic polarity of the iron would cease to exist. In this neutral condition, the iron, by its acquired momentum, would be carried past the plane of the horseshoe magnet, and the points across the partitions to the mercury on the other side. The moment those points re-entered the mercury, the electric current would again rush through the spiral, but its direction would be reversed; because that point, which, in the former case, was directed towards the zinc, would now be directed towards the copper, and *vice versa.* This sudden change in the direction of the current would cause a corresponding transiliancy in the magnetic polarity of the bar *n s*. Repulsion would now take place between the polar extremities which before attracted, and attraction between those which repelled; *n* would thus travel from S to N, fig. 32; or from *a*, in the direction of the arrow to *b*, fig. 30: and *s* would consequently travel from N to S, fig. 32; or from *b* to *a*, fig. 30. The points would now again cross the partitions, and succeed each other in the mercurial connexions. The electric current would thus be reversed in the coil, and the obsequious magnetic poles again change places in the revolving bar.

Similar vicissitudes of electro-magnetic action would take place at every transit of the points across the vertical magnetic plane; by which means the rotation would be maintained, whilst sufficient electric energy was exhibited by the voltaic battery employed. W. S.

XXII. *Two Brilliant Electrical Experiments, well calculated for the Lecture Table.*

The first of these experiments which we shall describe is made by the electrical machine and the apparatus represented in figs. 34 and 35, Plate V. the former being a front, and the latter a side elevation.

A A is a stout rectangular mahogany board, which is the base of the instrument. B B B is a vertical piece of similar board, the lower end of which is firmly fixed in the base. Near to the upper end of this vertical piece is a crutch C, fig. 35. which, together with the main upright B B carry a spindle with its pulley P. The spindle and pulley are put into rotatory motion by means of the wheel W W, and its band. The farther end of the spindle terminates in a hollow brass ball, into the opposite sides of which, and at right angles to the axis of the spindle, are cemented two glass tubes, spirally spotted with tin foil as seen in fig. 34. The outer end of each tube terminates with a small brass ball. By this arrangement the spotted tubes can be put into rapid rotation in a vertical plane. S is a glass pillar surmounted by a brass socket terminating upwards in a screw. On this pillar is screwed the ring o, o, o, made of stout brass wire. The inner diameter of the ring must be a little greater than the distance between the outer surfaces of the balls terminating the spotted tubes, in order that the latter may rotate within the ring without touching.

The axis of motion is in the centre of the ring and perpendicular to its plane. A horizontal wire *w*, terminating with a brass ball is screwed to, and projects from, one side of the ring.

When an experiment is to be made with this apparatus, the ball *w*, is to be brought close to the farthest extremity of the prime conductor of an electrical machine, in good order; or to a ball proceeding from the conductor: and in this position the base-board is to be screwed firmly to the table, in the usual way with clamps.

When the machine is at work, sparks will pass from the prime conductor to the ball *w;* and again from the inner side of the ring to one or both of the spotted tubes, which will thus be brilliantly illuminated, especially if the spindle be touched with the hand, or connected with the cushion by a copper wire. Let now the wheel W W be gently turned; the spotted tubes will still be illuminated; but instead of showing stationary spiral lines of fire, they will now exhibit

the most pleasing spectacle ever beheld in the whole range of electrical illuminations; whose fanta tic forms will undergo a vareity of changes with the speed of the wheel; and when the velocity is considerable, the optical illusion creates ideas of a complete disc of electrical light.

This splendid experiment is susceptible of much pleasing variation. If instead of having the tubes cemented into the central revolving ball they be fastened to it with screws, in the usual way of screwing balls on the extremities of wires; they may easily be removed and replaced by other devices, such as tubes of coloured glass spotted in the same manner: or by slabs of plate glass, spangled in the usual way and varnished with different colours. In this way a disc of any coloured light may be exhibited; or the luminous disc may be composed of concentric annular portions, each of a different colour. If, for instance, the faces of two revolving slabs of glass were each divided into three equal portions by lines perpendicular to their edges, and that the inner portion be varnished yellow, the middle red, and the outer portion blue, each portion could form an annulus of its own colour, and the whole would fill up the whole disc. In all experiments with this apparatus, however, the disc, whatever colour or colours it may exhibit, will necessarily appear annular, because of the central brass ball; which, in a darkened room, is a complete black speck.

By this apparatus differently coloured pieces of glass may be made to combine the light they transmit; and the composition of colours displayed in the most splendid manner.

The second experiment is performed by the aid of a magnetic-electrical machine, and the apparatus represented by fig. 36, Plate VI, which is an end view.

In all magnetic-electrical machines, made in London, the coil wires terminate in cylinders concentric with the axis of motion, or spindle, which carries the revolving cross-piece with its coils; which cylinders are imperfectly insulated from each other by an intervening cylinder either of box-wood, ebony, or some material of an imperfect conducting character.

In fig. 36, the central white space represents the end of the inner cylinder, usually a piece of stout copper, or brass wire, to which the inner ends of the coil wires are soldered. The shaded annulus next this cylinder is the hollow insulating cylinder of wood; and round it is seen the end of another unshaded annulus, representing the outer brass cylinder to which the outer ends of the coil wires are soldered. These cylinders fit one another pretty tight to prevent their moving out of their places.

Instead of making the usual connexions for showing the spark, a piece of iron wire is screwed into each metallic cylinder, pointed at the other end, and bent nearly at right angles, as seen in the figure. And when the machine is in motion, these wires are carried round in a vertical plane, in front of, and parallel to, an annular iron disc *i, i,* which is firmly fixed in the base-board of the machine by the pillar S. The points of the iron wires are directed obliquely towards the iron disc, against which they press gently during the whole revolution. By this means, the two cylinders, and consequently the ends of the coil wires, find a metallic connexion, which is frequently interrupted whilst the points are scraping over the asperities on the surface of the iron disc, and two concentric circles of brilliant scintillations are thus produced. This is, perhaps, the most pleasing experiment yet shown in magnetic electricity.

By this arrangement it sometimes happens that the revolving points do not succeed one another properly in the contacts with the iron disc in order to produce complete circles of scintillating fire. When this is found to be the case, we sometimes remove the longer wire from the outer cylinder, and connect the latter with the iron disc by a piece of copper wire, having a loop at one end for the reception of the cylinder, and bent into a hook at the other to hang on the inner edge of the annular iron plate. This wire, however, is a mere substitute for a spring of either steel or brass wire, one end of which is screwed tight to the back part of the iron disc, and the other presses against the outer revolving cylinder. This spring can be turned off the cylinder at pleasure, by slackening the screw in the iron disc; and replaced by the iron point as in the figure.

The iron disc *i, i,* may be either rough or smooth, bright or dull; and as much of the display depends upon the combustion of the ferruginous matter, steel points are preferable to iron ones. If the points be filed or ground very fine, each wire may carry two, at different distances from the centre of motion, which will usually produce four concentric circles of fire.

If instead of a steel point we use one of platinum, leaves of laminated metals, covering the face of the annular disc *i, i,* will be deflagrated. Gold, silver, and the Dutch metals, will, by this means respectively display the same characteristic coloured light as by the action of a voltaic battery. The metallic leaves will be sufficiently attached to the plate *i, i,* by first breathing on the latter and then pressing the former against it. In these experiments, however, it is necessary

that the plate *i i* be bright. It may be of iron or any other metal.

The deflagration of mercury is beautifully shown by an apparatus of this kind. Let the annular disc *i i* be of copper, and the whole face over which the points revolve slightly grooved with a file in close radiations from the inner to the outer edge. Let this radiated face be amalgamated with a dilute solution of nitrate of mercury, then washed in clean water, and afterwards covered with clean mercury so that the whole face may appear bright. Now remove the surplus mercury from the grooves with a soft tooth brush, or with a stout feather, and the disc is ready to be attached to the machine for an experiment. If the revolving points be amalgamated copper, two circles of brilliant white light will be exhibited. But if they be iron, or steel, the colour of the luminous circles will be a compound of the white light from the mercury, and of the fiery red from the deflagrating ferruginous points. If points of zinc, gold, silver, &c. be used, each metal will tend to modify the tint of the luminous rings. Thin wires of these metals, tied by binding wire, to the revolving wires, answer extremely well for the points.

W. S.

XXIII. *A general outline of the various Theories which have been advanced for the Explanation of Terrestrial Magnetism.*

The singularity and importance of the polar directive tendency of a magnetic needle, could hardly fail to attract the attention of philosophers in a very early period in the study of magnetism; and their curiosity thus excited would naturally lead to enquiries respecting the cause of so extraordinary a phenomenon, the privilege of whose display seems to be conferred on one solitary kind of matter only, dignified by nature with a susceptibility peculiar to itself, of a permanent retention of the arranging magnetic force.

The ancient philosophers, who were well acquainted with some of the mysterious operations of the magnet, have left no trace on record which can claim for them any knowledge whatever of its tendency, when unrestricted, to arrange itself in any particular direction. The directive propensity of the needle is a discovery of comparatively modern date; and an explanation of the cause of this remarkable phenomenon, as was reasonable to expect, has been attempted in a great variety of ways.

Some of the earliest hypotheses may be placed amongst the wildest chimeras that could possibly suggest themselves to the mind of man; or even obtrude amongst the fleeting fantastic imagery that skims the reposeless undulatory fancy. The more sober-minded philosophers, however, were enabled to trace the influencing power to the action of the earth, considered in the capacity of a grand natural loadstone, whose predominating magnetic forces reside in the polar regions; by virtue of which, the obedient needle is constrained to assume certain directions, according to its residentiary position with respect to the governing terrestrial magnetic poles, which had already been discovered to be situated, and supposed to be permanently fixed, at some distance from the poles of diurnal rotation.

This notable hypothesis, the production of sound analogical reasoning, was founded in the miniature attractions and repulsions exhibited by magnets on each other: phenomena easily traced to, and assimilated with, the action incessantly and universally displayed over every part of the surface of the earth, on a needle unreservedly submitted to its magnetical polarizing influence.

This plausible, and from the then stage of discovery, very natural, conclusion, was, for a long time, universally received as the true theory of terrestrial magnetism: but the discovery of the motions of the needle at the same place of observation became productive of unforeseen impediments in the way to truth, which required fresh efforts of the mind to reconcile the theory to the phenomena. Here, again, the natural concomitancy of ardent emulation and premature conclusions were manifested by many creations of vivid genius and fertile imagination, put forth as solutions to the new magnetic problem.

Descartes imagined that the transportation of iron from one place to another, and the growth of new iron within the earth, where there was none before, might be the cause of the needle's varying positions. Kircher tried the problem by multitudes of flexible magnetic fibres; and Canton attempted its solution by the aid of subterranean fires. Volcanoes and other restless agents, both real and imaginary, were resorted to, and marshalled in this clashing of opinions, to reinforce particular hypotheses, whose authors, ambitious for fame, appeared to be determined, at all hazards, to venture a *lift* of the terrestrial magnetic poles, by agencies which, even to themselves, must appear inefficient and preternatural.

The famous theory of Dr. Halley, which worked with four magnetic poles, and a revolving loadstone nucleus in the

earth, though, perhaps, as inaccurate as the rest, seems for a while to have had a considerable degree of ascendency; and if it did not completely paralize its predecessors, its ingenious machinery was at least eminently calculated to throw into the shade the phantom forces of subterranean fires, ferreous transportations, and all the perturbating agencies of former hypotheses; which, even had they existed in reality, would have been too irregular in their actions, and too limited and local in their performance, to assume the dignified character of the prevailing, steady, universal powers which govern the tranquil uniform motions of the magnetic needle.

The magnetic effects which were observed to be produced by lightning, and Franklin's discovery of the identity of the latter with electric discharges artificially produced, were sufficient materials for novel ideas respecting the nature of magnetic action; and the aurora borealis, also considered an electric phenomenon, and said to disturb the magnetic needle, gave an auxiliary impulse to fresh enquiries, now directed in search of the identity of electricity and magnetism. Philosophers were still further encouraged in this pursuit by the striking analogies which are so remarkably characterized in the attractions and repulsions of the two powers. Similarly electrized bodies display a tendency to recede from each other; and so do those which are similarly magnetic. Dissimilarly electrized bodies will mutually approach each other, and so will those which are dissimilarly magnetic. Thus far the analogy appears to be complete; but it must be confessed, however, that, notwithstanding the most curious phenomena have been elicited, and even a new science established by prosecuting these experimental investigations, not one step more has been gained towards identifying the electric and magnetic powers.

In the earliest enquiries in the search of electro-magnetism, philosophers employed every implement of investigation of which they could at that time avail themselves. The electric machine and powerful batteries of jars were the most efficient apparatus then known. The most formidable of these were now brought into requisition, and shocks the most terrible they could produce were communicated to steel bars of every shape and size that the experimenters could invent, to favour their views and shorten the path to discovery.

Success for awhile seemed to attend the labours of some of those arduous experimenters who were engaged in the pursuit, by the production of magnetic polarity in bars of steel submitted to electric discharges. The electric fluid in these experiments traversed the length of the bars; but the mag-

netic poles, thus produced, were soon found to have no reference whatever to the direction of the electric stream. A north pole would sometimes appear at that extremity which was directed towards the positive side of the jar, and sometimes at that placed towards the negative side; leaving the mind of the experimenter under the most unenviable impressions: on one hand, hope vanishing in the distance, and on the other hand, trembling on the brink of despair. Ultimately, however, it was discovered that the results of all these laborious investigations had no superiority over those usually produced by the blow of a hammer, or other mechanical action which could agitate to a sufficient degree, the particles of the steel bars; during the trembling motions of which, the polarization is facilitated by an enhanced susceptibility of arrangement of the residentiary magnetic matter, by the constantly and universally operating magnetic influence of the earth.

When a soft iron bar is placed in the position in which the dipping needle seeks to repose, its magnetic matter becomes arranged by the influence of terrestrial magnetic action; and its polarization is similar to that of the needle. This magnetic condition, however, is as transient as the position of the iron, and fluctuates in energy and locality with every movement of the bar. With hardened steel the polarization is not so easily accomplished. The energies of the earth are too feeble to vanquish the obstinate retention of the quiescent metal in which the magnetic matter is imprisoned. Tremulous agitation, however, from whatever source it may proceed, slackens the retentive powers of the steel, and liberates the obsequious fluid to the influence of terrestrial magnetic force. The vibrating bar thus becomes a magnet; the retention of whose polarity is progressively sealed by the gradual decline of its trembling motions. Such were the general results of a long and arduous experimental enquiry by the employment of the common electrical apparatus: the polarity produced in the steel by electric shocks being the proper effects due to terrestrial magnetic action: for that end of the bar which pointed towards the north during the electric discharge invariably received the same kind of polarity as the north end of the needle: and the south end of the bar, polarity of the other kind. Indeed, in some of the experiments of Father Beccaria, a famous Italian philosopher, in which the electric discharges were sent from west to east, and *vice versa*, the whole of the north side of the bar exhibited the one kind of polarity and the whole of the south side the other kind, still conformably to the laws which governed the results obtained from the other experiments.

Beccaria was one of the ablest electricians of his day, and took an exceedingly active part in this particular enquiry: his conclusions are very remarkable. "Are not these peculiar effects of the electric fire with respect to magnetism," said he, "so many proofs which corroborate my former conjectures, that the peculiar magnetic force observed in *loadstone*, is to be attributed to either atmospherical or subterraneous strokes of lightning: and that the *universal systematical* properties of magnetic bodies are produced by an universal systematical circulation of the electric element?" This is precisely the principle which, in fifty years afterwards, became the basis of Ampere's theory of all magnetic action.

The discovery of Galvanism in 1898 commenced the most splendid era in the history of electricity; and the invention of the Voltaic battery furnished philosophers with a new and formidable implement of electrical research. Electro-magnetism was again eagerly sought for by the aid of this novel apparatus; and, notwithstanding the numerous failures which attended some modes of its application, the experiments of the indefatigable Œrstead were eventually crowned with success.

This ingenious Danish philosopher in 1819 discovered that an electric current, traversing a metallic wire which joins the two poles of a Voltaic battery, will deflect an adjacent magnetic needle from its natural direction; and that, in the absence of interfering forces, the latter would be placed at right angles to the former, conformably to a certain law, which gives exact and undeviating positions, as regards the axis of the current and the poles of the needle: so that if a spectator were to look at the vertical conducting wire carrying an electric current from its superior to its inferior extremity, a magnetic needle placed posterior to the wire, and under the influence of the electric current only, would have its north end directed towards his right hand. And, because any side of the wire may be turned towards the needle without producing change in the phenomenon, the action is obviously alike on every side of the current.

This discovery again opened a new field of research, the fertility of which was soon manifested by the great variety of interesting facts which, by the emulative labours of experimenters, were speedily brought to light; nay, with a rapidity unparalleled in the history of science: but we must pursue those only which were the most remarkably turned to account in remodeling the theory of terrestrial magnetism.

Ampere, the celebrated French philosopher already noticed, made some capital discoveries in electro-magnetism. He had been led to conclude, from his reasonings on Œrsted's experi-

ments, that electric currents ought to exhibit some action on each other, which he soon found to be the fact. In a memoir which, on the 25th September, 1820, was communicated to the French Academy of Sciences, Ampere showed the conclusions at which he had then arrived, which are, as follow.

1. "Two electric currents attract one another when they move parallel and in the same direction, and they repel one another when they move parallel and in opposite directions.

2. "It follows, therefore, that when the metallic wires through which these currents are transmitted, can only turn in parallel planes, each of the two currents tends to bring the other into a situation where it may be parallel to it, and in the same direction.

3. "These attractions and repulsions are absolutely different from the attractions and repulsions of ordinary electricity.

4. "All the phenomena discovered by Mr. Œrsted, and which I analyzed, and reduced to two general facts in my first memoir, are embraced by the law of two electrical currents, admitting that a magnet is only an assemblage of electric currents, produced by the mutual action of the particles of steel, analogous to that of the elements of a voltaic pile, and which move in planes perpendicular to the line which joins the two poles of the magnet.

5. "When the magnet is in the situation which it tends to take by the action of the terrestrial magnet, these currents have a direction opposite to that of the apparent motion of the sun; and hence, when we place a magnet in a contrary position, so that the poles which point to the poles of the earth are of the same name, the currents will be found in the direction of the apparent motion of the sun.

6. "This law embraces the phenomena of the ordinary action of magnets.

7. "It embraces also the phenomena of terrestrial magnetism, by supposing electric currents in planes perpendicular to the direction of the dipping needle, and which move from east to west.

8. "There is no difference between the poles of a magnet than that one of them is found to the left, and the other to the right of the electric currents, which give to the steel the magnetic property."

With these electrical habiliments, Ampere has given the last fashion, in which the theory of terrestrial magnetism has appeared, which by the advantage of novel materials, unknown to Beccaria, is an ingenious renovation of the views taken by the illustrious Italian about fifty years before: and it is remarkable, that notwithstanding the adscititious ele-

ments introduced by Ampere, his theory, at the time it was framed, had no known advantages over its Italian predecessor. The electric currents in steel magnets are not, even now, known to have an existence; and no source of action had then been discovered, to which the requisite and idoneous terrestrial electric currents could be traced.

Ritter, an active, indefatigable, and exceedingly ingenious philosopher of Bavaria, as early as 1805, and consequently fourteen years previously to the discovery of electro-magnetism, had considered the earth in the capacity of a "vast voltaic pile, whose poles are on one side sufficiently closed by the waters of the ocean," and has said that every magnet is equivalent to a pair of heterogeneous metals united together: its different poles represent, as it were, different metals."

That electric currents are continually flowing in the earth must appear obvious to every one conversant with voltaic electricity. The materials which form our batteries, and display electric streams at our pleasure, have all been brought from this exhaustless source. Nature's laboratory is well stored with apparatus of this kind, aptly fitted for incessant action, and the production of immense electric tides; and the insignificancy of our puny contrivances to mimic nature's operations, must be amply apparent when compared with the magnificent apparatus of the earth.

That local electric currents do absolutely traverse the interior of the earth has been amply manifested by the experiments of Mr. Fox, and Mr. Henwood,* in the Cornish mines; but these facts were unknown at the time Ampere framed his theory: and that there are continual electric currents from east to west, under the surface of the earth, was then as problematical as the existence of similar currents in every magnet of steel, remains at this day.

The circumstance which has given the most plausible appearance to Ampere's electro-magnetic theory of the earth, is the posterior discovery of thermo-electricity by Dr. Seebeck, by which it is shown that electric currents can be produced in bodies by an unequable temperature of their different parts. This simple exciting principle transferred to the vast apparatus of nature, displays the most magnificent theory of terrestrial magnetism that the mind can possibly conceive. The *Sun* is now the exciting agent, whose uniform tide of heat sweeping the tropical zone becomes the grand cause of a westerly circumflowing electrical flood: and thus converting the terrestrial ball into a thermo-electric magnet.

* See the next very interesting Article. Edit.

XXIV. *On the electric currents observed in some metalliferous veins.* *By* W. J. Henwood, Esq. *F. G. S, London and Paris. Hon. M. Y. P. S., Assay Master of Tin, in H. M. Duchy of Cornwall.*

To William Sturgeon, Esq. &c. &c. &c.

Sir,

It is my object in the following pages to describe briefly the geological features of the district (Cornwall) in which most of the experiments on this subject have been made, the method of observing which has been pursued, the results obtained, to enquire into the probable causes of the currents observed, and to examine whether the theories which have been erected on them be well founded.

The discovery of electric currents in some of the metalliferous veins of Cornwall, is well known to have been made by Mr Fox (*Phil. Trans.* 1830, p. 399.); it was soon followed by a beautiful experiment of Mr. Barlow's, in which the phenomena of the dip of the magnetic needle were well approximated, by suspending a magnetized bar over various parts of a hollow globe, around which voltaic electricity circulated through wires of copper placed in the parallels of latitude (*Phil. Trans.* 1831, p. 99). These discoveries must have been regarded as very confirmatory of M. Ampere's theory of the electric origin of terrestrial magnetism.

The mineral veins of Cornwall traverse both the granite and slate rocks without interruption; they are highly inclined tabular masses of great extent; on a small scale, exhibiting numerous curves and irregularities, both in direction and dip, with very variable breadths; but on a large view having an approximation to rectilinearity. Their composition is chiefly quartz with other earthy minerals, in many places mixed with metallic substances, viz. copper and iron pyrites, vitreous copper ore, oxide of tin, blende, galena, with admixtures of small quantities of other minerals; as native copper, red oxide, carbonates of copper, salts of lead, &c.; all frequently so intimately and indiscriminately mixed (mechanically) with the rock-contents of the vein, that their separation is among the most difficult and expensive of our mining operations.

These irregularly distributed masses, veins, granules, crystals, and other forms of the ores, have usually a prevailing dip longitudinally through the vein itself (*stuts or shoots*); and this is almost universally *from the granite and towards the slate,* whichever of them may be the containing rock. It

is also most commonly the case that the vitreous copper ore occurs in those parts of veins which are in the granite, and in the massive slate rocks (greenstones) contiguous to it; whilst the copper pyrites more usually occurs in the *lodes* when in the shistose members of the slate series; not however without many exceptions to both.

Tin ores abound more in the granitic than in the slate districts, notwithstanding very large quantities of it have occurred in the latter. It is a generally recognized fact that although the most perpendicular veins are not always the richest, yet the most productive parts of a given *lode* are those nearest to perpendicularity.

It is also true, but with many exceptions, that in parallel *lodes* the ores generally occur near the same N. and S. line. The same vein is, however, seldom productive in both granite and slate; notwithstanding parallel veins are often rich in similar ores in different rocks; thus the veins of Wheal Vor and Great Work mines are worked in both granite and slate, but the former is productive in the last mentioned rock, and the latter mine in the first named. Again, the copper ore at Tresavean is all in granite, whilst in the parallel veins of the adjacent United, Wheel Squire, and Ting Tang mines, it is entirely in slate.

The general *direction* of the principal metalliferous veins is about E. and W. (magnetic), but there is a second series, the *contra lodes*, bearing about N. W. and S. E. Both these systems are usually intersected and often dislocated *(heaved)* by a third series, the *cross courses*, which strike about N. and S. The *dip* will, of course, in either case, be at right angles to the direction, but sometimes to one side, and at others to the other; the *lodes*, for example, sometimes dipping to the N, sometimes to the S. The directions more or less coincide with the lines of symmetrical structure, by which both series of rocks are divided.

There is, however, one rather extensive district, that of Saint Just, in Cornwall, in which the metalliferous veins bear about N. and S. and the *cross-courses* (guides) about N. E. and S. W.

The extensive mining operations in this County afford excellent opportunities for examining the *subterranean temperature*. From an extensive series of observations I have satisfied myself that at all depths yet attained here, the slate rocks are on an average from two to three degrees warmer than the granite at the same level. *(Records of General Science* IV. 198.*)*

Of the mineral contents of the water from some of the mines, Mr. Fox says, *(Cornwall Geol. Trans.* III. *p.* 323.*)*

"I have examined the water from the bottom of several deep mines, and find it generally to contain very little foreign matter, not exceeding one to five or six grains in a pint. Its relative purity seems to have no reference to the temperature, or the depth of the mines; for instance, the deposit from the water, taken from the two deepest mines in this County, (each nearly 250 fathoms deep) Dolcoath and Huel Abraham, after evaporation, did not in either case, exceed two grains from a pint. On the other hand, the water from the Consolidated mines, when evaporated, left ten grains of residuum from a pint; from Huel Unity, sixteen grains; from one shaft of Poldice, nineteen grains; and from another, ninety-two grains from the same quantity.

"The muriatic salts, especially of lime, were most abundant; but in some instances I have detected common salt, particularly in the water from the bottom of the United Mines, the Consolidated Mines, Huel Unity, and Poldice. Out of the 92 grains of residuum from the latter, as mentioned above, 24 grains proved to be muriate of soda, 52 grains of the muriate of lime and magnesia, and the remainder, muriatic acid with iron, and sulphate of lime. The water from another part of the same mine contained 5·5 grains of common salt. All these mines are in killas, or primitive slate, and several miles distant from the sea;" (excepting Dolcoath and Huel Unity, which are worked both in granite and slate).

This terminates the brief notice of the geological features of the district, and its other subterranean phenomena bearing on our investigations.

The mode of experiment pursued was by pressing plates of sheet copper, of about 18 or 20 inches long, and from 3 to 4 inches wide, closely against such portions of the metallic contents of the vein as were thought proper for examination. To each of these plates a copper wire of 0·05 inch in diameter was connected by closely twisting it round; the opposite ends of each wire being connected in the usual manner with a galvanometer. In some cases but a few feet of wire were required to connect the stations; in others, many (in one instance six hundred) fathoms were employed. In many instances the points were situated at the same depth, on a continuous vein; in others, still at an uniform depth, and on the same vein, but on opposite sides of a *cross-course* dislocating (*heaving*) the vein; in some, too, the stations were at the same level, but on different veins; whilst there were many occasions in which the same vein, and others, in which different veins, at different depths were connected, through the galvanometer. The experiments have been made on metal-

liferous veins bearing E. and W., N.E., and S.W., and N. and S.

The results obtained were much the same, whatever were the directions of the veins. In all those producing *tin ore* alone, in many which afford copper, and in most cases where there was a continuous mass of copper ore between the points examined, no electricity was detected. In some instances, however, where all the intervening space consisted of rich copper ore, most energetic action was detected. The general fact, however, appeared to be, that in such cases the currents were most feeble, and where the continuity of a mass of ore was broken, either by an unproductive part of the *lode* itself, or by a *cross-course*, the currents were greatest. Copper pyrites, vitreous copper ore, black copper ore, galena, and blende, were among the contents of the veins in which the largest development of electricity obtained. But it is in the *metallic parts* of the veins *alone, in which these currents have been detected;* for, *notwithstanding the same means were employed*, both *on the earthy contents of the lodes and* ON THE ROCKS THEMSELVES, (as in the tin veins) *no trace of electricity has in any one case been discovered in either of them.*

The *lead veins* in the carboniferous limestone of North Wales, were also found by Mr. Fox, (*Cornwall Geol. Trans.* IV. 23.) destitute of electric currents. Herr Von Strombeck has repeated the experiments in some E. and W. veins, traversing the clay state, and Granwacke slate near St. Goar on the Rhine; the contents of one vein were copper pyrites, grey copper ore, and galena; and of the other carbonate and phospate of lead, grey copper ore, brown iron ore, and a little blende, spathose iron, and galena. In neither case however, could he detect traces of electricity. (*Archiv fur mineralogie, &c. Von Karsten* VI, 431, *and Bulletin de la Societe Geologique de France* V. 53.)

In his announcement of the discovery Mr. Fox said, (*Phil. Trans.* 1830, *p* 399.) "the direction of the positive electricity was in some cases from East to West, and in others from West to East; and when parallel veins were compared, its general tendency was, I think, from North to South, though in several instances it was the reverse. In veins having an underlie towards the North, the East was commonly positive with respect to the West; but in veins dipping towards the South, the contrary was observed, with one exception only, and that under rather unusual circumstances. In comparing the relative states of veins at different depths, the lower stations appeared to be negative to the upper: but exceptions

sometimes occurred when a cross vein of quartz or clay intervened between the plates, and the higher one was on the negative side with respect to horizontal currents."

I had the honour to assist Mr. Fox in the first experiment instituted, and of making many of those detailed in his first paper on the subject, as he mentions; I have subsequently extended them to mines in all parts of Cornwall, making in all 57 different series of observations, of which 45 were either altogether or in part conducted by me: and the following is a brief generalization of many of the results.

Direction of Vein.	Dip.	Direction of Electric Current and No. of Observations.	
East and West	North	East to West	18
Ditto	Ditto	West to East	6
Ditto	South	East to West	1
Ditto	Ditto	West to East	35
North-west and South-east	South-west*	North-west and South-east	1
Ditto	Ditto	South-east to North-west .	8
North and South	East	North to South	1
Ditto	Ditto	South to North	3
Ditto	West	North to South	1

when points at different depths were connected, in 13 cases the currents were *upward;* and in 35 *downward.*

In 36 experiments the direction has been *towards;* and in 21 *from* the granite.

The causes exciting the currents will be our next object of enquiry.

In the Reports of the British Association (III. *p.* 118.) Mr. Christie has made an objection to the conclusions drawn by Mr. Fox, which I had previously often urged on that gentleman, viz., that the wires employed, might by contact with the ores, have generated the currents observed. Mr. Fox has, however, entirely obviated it, by a very well contrived experiment, in which the copper plates were sometimes alternately employed with others of zinc, and in some instances zinc alone were used: the directions of the currents were the same in all cases. (*Reports, Brit. Assoc.* IV. 572.)

But two opinions have, so far as I know, been yet formed as to the origin of these currents, one, that they are *thermo-electric*; the other, *voltaic.* Both these were alluded to, by Mr. Fox, who however, *now* seems to prefer the latter, and this also appears to be the opinion of Strombeck. If *voltaic,* they must, of course, be accompanied by chemical decompositions; what substances, then, undergo these charges under

* There have been no observations on any of this class having a North-easterly dip.

ground? Our iron pyrites is a bi-sulphuret and very little liable to decompose under ordinary circumstances; and if oxygen were obtained for the purpose we should still have an excess of sulphur to dispose of, of which we find no trace in our veins. The ores of copper, zinc, &c., it may be said, are changing their forms; why then, when broken out of the lodes are they so constantly "unsullied and bright?" We should have expected that decompositions generating such enormous quantities of electricity, would have produced a more visible effect on their surfaces.

But we have seen that sulphuric salts have not been detected in the mineral waters of Cornwall; and whoever has descended into the mines has never been struck by the chemical changes going on; the sulphate of copper which in such a case should have been an abundant product, is scarcely ever met with. It is true that atmospheric influence, aided by the percolating water, and a rather elevated temperature, would probably induce many chemical changes, but where is the evidence of their progress?

As a greater development of voltaic electricity would accompany an elevation of temperature,* and as the heat increases as we descend, it would follow that the most deeply seated currents should be the most energetic, but nothing of the kind occurs.

We are also in the dark as to the mineral contents of the water in different rocks, and it certainly appears that no general law regulating their existence and nature, has yet been discovered.

It has, however, been shown that the various ores of copper are commonly contained in rocks of different characters, and this would indicate the probability of electric currents being excited, even if the liquid be the same in both cases.

All metals and many ores, among the latter the sulphurets of copper and lead, and iron pyrites, when unequally heated, are traversed by currents of electricity of very low intensity, although in considerable quantity.

In a communication to the Royal Geological Society of Cornwall, in 1834, Mr. Fox states, that when heated, copper pyrites was positive both to purple and vitreous copper ore, to galena, and to iron pyrites; also that elevation of temperature, induces *positive* electricity in the sulphurets of copper and lead, but *negative* in iron pyrites.

It is well known "that foreign metals brought into contact

* Faraday, 3d. series, Phil. Mag. VI., 414.

with a homogeneous circuit near the point of heat, participate in the action, and tend to determine the current." (*Prideaux Phil. Mag.*, 3*d. series*, 1833, III. *p.* 272.) Seeing the complicated results, obtaining when a *few metals only* are employed, could we expect simplicity where so many more elements enter our circuit?

We have seen that the contents of our veins consist of several ores of many metals: who would venture to predict the direction of a thermo-electric current excited in such a mass, the elements varying in the numbers, magnitudes, and directions of their masses; in some places connected, in others disjoined; here, mixed; there, separate; in one spot, coincident; in another, opposing; parallel, in some situations; transverse, in others? It is obvious that the higher temperature of the slate, than of the granite, and of masses of ore dipping from the latter towards the former, are highly favourable conditions for the development of *thermo-electricity;* and in order to see how it accords with facts, we will *assume* a case which shall involve them all.

Suppose a mass of copper ore originating near the surface in slate, to be prolonged in extent to, or into, the granite; the upper portion would probably be copper pyrites, the lower, vitreous copper ore. On a given *horizontal* line, we shall then have the *positive* ore (the pyrites) in the *warmer* rock, and the *negative* (vitreous) in the *colder;* and the same result will obtain, whether each ore singly had been *unequally* heated, or whether *both* are in contact at the *same* temperature. In either case a current would be determined *towards* the granite.

If, however, we take a *vertical line*, our conditions are different; for here, if the points be distant, the *higher temperature of the vitreous* copper ore may render it positive to the copper pyrites. In some cases, it is obvious, differences of depth may compensate for the natural inequality of temperature between the granite and slate, and thus currents may be excited in contrary directions to those which would obtain, if both ores were *equally* heated in contact, or if the pyrites were warmer than the vitreous copper. It is evident too, that in many cases "foreign ores" may determine the directions of the currents; and that minor circuits may neutralize each other.

It is hopeless to search for any relation between the directions of the currents and the points of experiments; for these, although partaking of the natural temperature of the spot, are constantly modified by the combustion of candles and

gunpowder, the presence of workmen, and the circulation of air from other parts of the excavations, to all which influences the outer film of ore must be very obedient.

The *exciting* differences of temperature must be sought in the *mean* temperatures of the *sums of the masses* under experiment; and to them our observations being but *approximations* can be but imperfect indices.

The last division of our subject relates to the opinions which are supposed to be supported by these experiments, and the theory which has been founded on them.

Taken in conjunction with the beautiful experiment of Mr. Barlow, they would *prima facie* be thought confirmatory of the opinion that terrestrial magnetism is of electric origin. But it has been shown that these currents have no uniformity of direction, even in parallel veins; and that they exist of equal force in veins bearing N.W. and S. E. and N. and S. as in those having an E. and W. direction; remembering, too, that we are entitled to speak with but little certainty of Cornwall, whilst of all other parts of the world we can yet but guess.

If the electrical currents yet discovered have any effect on a magnetized bar freely suspended, they would of course stand at some angle to each other; in the mines of St. Just, however, the currents are *parallel* to the magnetic meridian.

But the discovery of electric currents in the *present contents* of our veins has been made use of as a foundation for a theory of the origin of the veins themselves. Professor Sedgwick, in one of his eloquent and admirable addresses to the Geological Society, says, "After the important experiments of Mr. Fox, there can, I think, be no doubt that the great vertical dykes of metallic ore which rake through so many portions of the County, owe their existence, at least in part, to some grand development of electro-chemical power."

I confess I see nothing in the experiments to bear up such an opinion, which would surely derive much stronger support from the observations of M. Becquerel, which had then been some years before the world.

This most original and ingenious experimenter, by the use of a curved glass tube, divided at the turned part by a bit of moist clay, and filling each leg of it with a solution of a different substance, which he connected by a piece of wire, obtained *crystals* of metallic copper, red oxide of copper, and vitreous copper; metallic silver, sulphuret of silver, galena, sulphuret of antimony, and many other substances, the agency being *the electricity developed by the solutions alone.*

The theory which Mr. Fox has recently brought forward,

assumes the previous existence of fissures in the strata, which are subsequently gradually opened; the contents of the fissures he imagines to have been deposited by electric currents generated by the actiou of saline solutions on the rock masses.

The opinion of a gradual opening on the line of previously existing fissures, was propounded by Werner; (*Theory of Mineral Veins, English Translations*, 68.) and I purpose the task of examining whether it is consonant with the phenomena of this County in another place; at present we have only to deal with the electric part of the theory.

The discovery of electric currents in the *present contents* of veins, appears to me to have no necessary connexion, with the mode of their original deposite; and if it had, there are not only *no currents in the tin lodes,* but many of the lead and copper veins are equally destitute of them.

The direction of the metalliferous veins of St. Just, is parallel to the magnetic meridian; and in this series the currents are supposed to have acted in preference to the line of the *cross veins* (guides) which is about N.E. and S.W.; yet it is admitted that currents will most *readily pass at right angles* to the magnetic meridian, and this *facility* is made the *reason* for the E. and W. and N.W. and S.E. veins being so filled in preference to those bearing N. and S.; but still all the metalliferous deposite are supposed to be synchronous.

Beside this, it must not be forgotten that the magnetic *variation* is a fluctuating quantity; and that, therefore, that which is *now* parallel or at right *angles to it,* was not so, *some time since,* and will not be so *a little hence.*

One rock is assumed to be positive and the other negative, (Mr. Fox has not said which he supposes in either state), and thus one ore is deposited in the granite, another in the slate.

We have seen that in the contiguous mines of Huel Vor, and Great-work, all the tin ore in one mine is in slate; and all the like ore in the other is in the opposite rock; also that in Tresavean, the copper ores are *all in the granite,* whilst in the parallel veins of United Mines, Wheal Squire, and Ting Tang, they *are entirely in slate.*

But we are told by Mr. Fox, that different saline solutions, existing " in the rocks are capable of exciting voltaic action, and giving rise to voltaic currents, even if there were no other cause sufficient to produce them." (*Report of a lecture delivered at Redruth, West Briton, Nov.* 4, 1836.)

I have already stated that the currents detected in veins have been *confined to their metallic portions;* and that notwithstanding the *same means were applied to the* ROCKS, *and*

the EARTHY CONTENTS OF THE VEINS, NOT A TRACE OF ELECTRICITY HAS YET BEEN FOUND IN EITHER.

Mr. Fox does not allude to the original locality of the metals thus supposed to be deposited by the currents; this is, however, a very necessary point of enquiry, for they may have been beyond the reach of electricity. If, however, they are supposed to have been in solution, it is but an obvious application of M. Becquerel's experiments; where, however, are the solvents now? If the currents be assumed to have acted IN the *fissures* only, what force *collected* the matter which was not originally *contained in them?*

If the force acted *beyond* the fissures, why were not the ores deposited *out of them?*

I regard these currents as merely local, originating probably in inequalities of temperature prevailing among *different ores*, in *various rocks*, at different depths; possibly also in voltaic combinations of various metallic substances. These may be adequate to the induction of chemical changes within certain limits, but that they perform any more important function in the economy of nature I do not believe.

I have the honour to remain,
Sir,
Your very faithful humble servant,
W. J. HENWOOD.

4, Clarence Street, Penzance,
December 6, 1836.

P.S. It is still an object to be determined by experiment, whether these currents have any decomposing, or heating power, or whether they will yield a spark; I have long contemplated making the examination, but have been hitherto prevented by more pressing occupations.

XXV. *Experiments illustrative of the influence of Voltaic Electricity on Copper Pyrites. By* R. WEAR FOX, ESQ.*

A, fig. 33, Plate V., may represent a trough of wood or earthenware: *b*, a mass or wall of moistened clay firmly pressed down, and dividing the trough into two water-tight cells or com-

* The experiment here described is that which excited such intense interest at the Bristol Meeting of the British Association. The description has been politely communicated to us by Mr. Fox, at our particular request. This interesting experiment having been so variously reported in different Journals, we were certain that a correct description of it, direct from its Author, would be very acceptable to our readers. *Edit.*

partments; *c*, a piece of the native yellow or bisulphuret of copper in a solution of sulphate of copper; *z*, a zinc plate in *water*,* and connected with the copper ore *c*, by means of the copper wire *e*.

In a very short time the yellow copper ore became beautifully iridescent, resembling the "*peacock*" copper ore of the miners. It afterwards seemed to pass into the purple ore, (Buntkupfererz) and ultimately into the grey copper ore. This change was not merely superficial, but penetrated to some little depth under the surface when left in action for some time, perhaps two or three weeks. Metallic copper in brilliant crystals, (mostly octohedrons) was deposited on the ore, care having been taken to throw into the cell pieces of sulphate of copper from time to time as it seemed to be required. It appears that one portion of the sulphur becomes combined with the oxygen of the oxide of copper in solution, forming sulphuric acid, which passes through the clay and combines with the zinc. I have also substituted for the zinc a piece of grey copper ore, and found that grey and yellow copper ore acted upon each other with sufficient energy to change the surface of the latter into grey copper ore, and to deposit some metallic copper upon it. When these ores, (the grey and the yellow sulphuret of copper) are separated merely by clay, moistened with water taken from a mine, a galvanometer will show that there is a decided voltaic action between them. These experiments owe their chief interest to the circumstance of their bearing on some phenomena which our copper veins present; such, for instance, as the occurrence of metallic copper frequently in contact with grey or black ore, and not with yellow copper ore; and also of grey ore being usually found nearer the surface than yellow; and likewise in or near cross-courses and situations in which it may have been most exposed to the action of water, and the saline matter which it may have contained.

I have likewise found that when the muriate of tin is submitted to voltaic action, a portion of it is collected in the state of a peroxide at the positive pole; whilst another portion appears in a metallic state at the negative pole. This experiment appears to explain why we find tin and copper ore generally so distinct from each other, in opposite parts of the same vein, or in different veins, and very often one of these metals is found in a vein whilst traversing granite, and the other whilst it traverses "*killas*."

* I at first used a little sulphuric acid with the water, but the latter only will answer well.

XXVI. *A brief Account of a Visit to* ANDREW CROSSE, ESQ. *of Broomfield, on the Quantock Hills, Somersetshire, in September,* 1836, *communicated by* SIR RICHARD PHILLIPS *to* DR. MANTELL, *at Brighton, and read at the Conversazione of the Mantellean museum, Tuesday, Sept.* 20. *Corrected and enlarged by the Author for the Annals of Electricity.*

While hundreds as well as myself were listening in the Geological Section, at Bristol, to the manly eloquence of Dr. Buckland, the pure intelligence of Professors Sedgwick and Phillips, the elaborations of President Murchison, the shrewd reasoning of M. de la Beche, and the honest enthusiasm of Mr. Conybeare, we were further amused by a very original experiment of Mr. Fox,† of Penzance, in which, by galvanic action, he had in twenty-four hours converted one state of copper into another state, by an extemporaneous apparatus in a blacking pot; and we were then surprised by an observation of Dr. Buckland, that he had now to introduce to the section a philosopher who had made great discoveries by the use of a brick with a hole in it, immersed in a pail of water.

Mr. Crosse then presented himself, and after laughing at the Doctor's description of his apparatus, began a modest and unprepared account of the results of his experiments on the conversion and production of mineral substances, in which he had been engaged many years. He stated that he had extensive voltaic batteries at work, by which he had formed quartz, arragonite, malachite, &c.; but that, as such formations were slowly produced, so he had latterly used no acid in his combinations, but only pure water. He detailed various results of different experiments, some successful and some failures, and in his impassioned descriptions of the latter he created much merriment.

The most lively bursts of satisfaction proceeded from all who were present during these details; and Professor Sedgwick then announced his recognition of Mr. Crosse, as an electrician, the magnitude of whose experiments had surprised him during a mineralogical excursion in the Quantock Hills about seventeen years before.

All that passed, as well as the rustic appearance and manners of Mr. Crosse, created an opinion that he was a very obscure individual, who, in an original and untaught manner,

† See Mr. Fox's description of his interesting experiments. EDIT.

had pursued the subjects of electricity and galvanism with the limited views of his class of experimenters. Nevertheless, he gave a general invitation to all who chose to visit his retreat in the Quantock Hills, and expressed his readiness to show them his apparatus, and the present state of his experiments.

The originality of the circumstances determined me at once to accept his invitation, and the day after that on which the business of the association was finished, I proceeded to Bridgewater, from which Broomfield is distant about eight miles in the hill country.

In my route I could not avoid turning my attention to those phenomena of atoms which result from the disturbance or excitement called electric, and from their restoration to their usual fit state of co-existence, with a force equal to the exciting force, but capable of condensation into burning and dispersing points.

Since I experimented with electricity, 48, 47, and 46 years ago, I have always regarded it as a mechanico-chemical disturbance of the atoms previously combined in *fit* and *necessary* relations in an electric stratum or space, when bounded or stopped by a surface which is a non-electric. I regarded the atoms so electrically arranged as the same atoms which, under other modes of excitement, produce tones in music or colours of light.

It would seem as though every condensation of oxygen by heat or otherwise (for oxygen seems always to follow motion or heat) creates a necessary flow of hydrogen to the opposite part; for in combustion we use materials that afford a supply of hydrogen, and therefore every example of flame may be deemed only a variety of the electric spark. We may, however, reason conversely on the same principle of necessary equilibrium; for as the force of union is reciprocal, we may have flame by raising the hydrogen of a combustible into gas, and then imagine a necessary flow of oxygen as the cause of flame or sparks, by its union, fixation, and reduction of volume.

In philosophizing on these atomic subjects, we must remember that atoms of gases, as such, have motions and forces of their own, nearly or quite independent of the diagonal accelerations of the two terrestrial motions called weight—that solids, chiefly affected by the last, are patients of actions and re-actions of atoms only as they obstruct, or, in constituents, sympathize. In varied circumstances, we may then solve mysteries which otherwise resemble caprices of flirtation. Metals in general seem impervious to these atomic actions: and the father of metals, either in its structure or atmosphere,

exhibits, in connection with its weight, a complexity of appearances solvable only on principles of analogy.

In accordance with these views of the subject, we see why, in every case of electrical display or action, positive and negative must always exist together—why electricity must always be thought of as a combination of positive and negative—why it is a solecism to speak of electricity as connected positively or negatively with any single body—and, in a word, why, if we think at all, or know the letter A in the alphabet of its science, we can in no way contemplate it but as a combined whole in its positive end or side, and in its negative end or side. In this view the wonder about distant induction within disturbed electrics, ceases.

An electric, as air, glass, &c., is a space containing the elementary atoms susceptible of separation, and of becoming by the separation capable of displaying contrasted action on its two sides. A non-electric is a body whose elements cannot be separated by this mode of action; and it is therefore, as it is more or less insusceptible, *an obstructor or stop* to the action in the adjoining electric; and in that case the stratum of the electric is spread over it by the perpendicular reaction of each obstructing surface.

We have now to consider what takes place on the restoration of a volume of the atmosphere or electric. An undisturbed volume of atoms was previously combined with certain natural fitness and force; but in all electrical excitement the usual arrangements are *disturbed* by some force which deranges the equilibrium, and an equal re-acting force therefore exists between the two sides of a plate by which they seek to re-unite. Hence the attractions and counter-attractions of light bodies between two plates in contrary electricities. When left at rest, restoration is an effect of the action and re-action of the surrounding air and the disturbed plate. But we more commonly restore the electrical strata to the equilibrium of comixture, by the direct communication of a spark through a projection, which, in diminishing the distance, assembles the force of an extended surface in its diminished dimensions. In an electrified volume we may conceive of the strata nearly as of the strata of the earth, except that in the electrified strata there is an active force exerting itself to leave the stratified form and return to co-mixture.

But Mr. Crosse works with the voltaic apparatus, and ought we not rather to consider that? To this I reply, that electricity and galvanism are identical, and a confirmation of what has been previously stated. We put between plates of different powers of oxydation, acidulous compounds with water—

we get electrical action directly, as one of the plates oxydates for our positive pole; and we obtain a generation of hydrogen on the other plate for the negative pole. The disturbed surfaces are then united by a better continuator than the fluid between the plates, and the opposite conditions proceed along wires to restoration. The currents, where their proximity increases, then *force parts of bodies placed in their course*, each its contrary element, and hence all the miracles of voltaic poles. In general a violent dispersive power has been employed, and electricians have laid their glory in the force and despersive effect of their apparatus; but Mr. Crosse has had the wise and modest ambition to employ a low power, so as in *long time* to generate and combine instead of destroying and dispersing. In this variation he has happily studied the processes of nature, and has in consequence been able to imitate them.

On reaching the handsome mansion of Mr. Crosse, situated in an undulating park, studded with trees of great bulk and age, I was received with much politeness, and found that I was the first visitor from Bristol. As I was preparing to retain my conveyance, to convey me back to Bridgewater, I was requested to return it, and pressed to stay to dinner and take a bed. Breakfast being well served, Mr. Crosse then conducted me into a large and lofty apartment, built for a music room, with a capital organ in the gallery; but I could look at nothing but the seven or eight tables which filled the area of the room, covered with extensive voltaic batteries, of all forms, sizes, and extents. They resembled battalions of soldiers in exact rank and file, and seemed innumerable. They were in many forms—some in porcelain troughs of the usual construction; some like the *couronnes des tasses;* others cylindrical; some in pairs of glass vessels, with double metallic cylinders beside them; others of glass jars, with strips of copper and zinc. Altogether there were 1,500 voltaic pairs at work in this great room, and in other rooms about 500 more. There were, besides, other 500 ready for new experiments. In all, 2,500 pairs. It seemed like a great magazine for voltaic purposes!

There are also two large workshops, with furnaces, tools, and implements of all descriptions, as much as would load two or three waggons.

In the great room there stands a very large electrical machine, with a 20-inch cylinder, and a smaller one; and in several cases were all the apparatus in perfect condition, which are described in the best books on electricity. The prime conductor stood on glass legs, two feet high, and there was a medi-

cal discharger on a glass leg of five feet. Nothing could be in finer order, and no private electrician in the world could, perhaps, show a greater variety, both for experiments and amusement.

Beneath the mahogany cover of a table, on which stood the prime conductor, &c., was enclosed a magnificent battery of 50 jars, combining 73 square feet of coating. Its construction, by Cuthberston, was in all respects most perfect. To charge it required 230 vigorous turns of the wheel, and its discharge made a report as loud as a blunderbuss. It fuses and disperses wires of various metals: and the walls of the apartment are covered with framed impressions of radiations from the explosion taken at sundry periods. Mr. Crosse struck one while I was present, and he has promised me one as an electrical curiosity and a memento of my visit.

But Mr. Crosse's greatest electrical curiosity was his apparatus for measuring, collecting, and operating with atmospheric electricity. He collects it by wires, the 16th of an inch diameter, extended from poles to poles, or from trees to trees in his grounds and park. The wires are insulated by means of glass tubes well contrived for the purpose. At present he has about a quarter of a mile of wire spread abroad, and in general about the third of a mile. A French gentleman had reported to the section at Bristol that the wires extended twenty miles, filling the entire neighbourhood with thunder and lightning, to the great terror of the peasantry, who in consequence left Mr. Crosse in the free enjoyment of his game and rabbits. This exaggeration Mr. Crosse laughed at most heartily, though he acknowledged that he knew that no small terror prevailed in regard to him and his experiments.

The wires are connected with an apparatus in a window of his organ gallery, which may be detached at pleasure, when too violent, by simply turning an insulated lever; but in moderate strength it may be conducted to a ball suspended over the great battery, which being connected with it, is charged rapidly, and is then discharged by means of an universal discharger. He told me that sometimes the current was so great as to charge and discharge the great battery 20 times in a minute, with reports as loud as cannon, which being continuous were so terrible to strangers that they always fled, while every one expected the destruction of himself and premises. He was, however, he said, used to it, and knew how to manage and controul it; but when it got into a passion he coolly turned his insulating lever, and conducted the lightning into the ground. It was a damp day, and we regretted that our courage could not be put to the test.

Every thing about this part of Mr. Crosse's apparatus is perfect, and much of it is his own contrivance, for he is clever in all mechanical arrangements, and has been unwearied in his application almost night and day, for 30 years past. I learned, too, that in the purchase and fitting of his apparatus, he has expended nearly £3000, although in most cases he is his own manipulator, carpenter, smith, copper-smith, &c.

About 12, Professor Sedgwick arrived, and in the afternoon one or two others, besides seven or eight gentlemen of the neighbourhood, who had been invited to meet us at dinner; for Mr. Crosse unites to the rank of Esquire that of a county magistrate, in the duties of which he is respected alike for his humanity to the poor and for his liberal opinions in politics. Mrs. Crosse I had not the pleasure of seeing, one of the sons being ill. Mr. Crosse himself was educated at Oxford, and his second son holds the living at Broomfield. He is master of all his father's experiments, and, in spite of the complaints of an Oxford education, I found him to be a very expert mathematician, well read, and variously accomplished. At seven o'clock we enjoyed a dinner as well served as I ever saw in any state dinner in London, and beds being reserved for Professor Sedgwick and myself, we next morning renewed our survey, previous to fresh arrivals, and I took notes of every thing connected with his aqueous voltaic batteries, in the following order, errors excepted:—

1. A battery of 100 pairs of 25 square inches, charged like all the rest with water, operating on cups containing 1oz. of carbonate of barytes and powdered sulphate of alumine, intended to form sulphate of barytes at the positive pole, and crystals of alumine at the negative.

2. A battery of 11 cylindrical pairs, 12 inches by 4. This by operating six months on fluate of silver, had produced large hexahedral crystals at the negative pole, and crystals of silica and chalcedony at the positive.

3. A battery of 100 pairs of four square inches, operating on slate 832, and platina 3, to produce hexagonal crystals at the positive pole.

4. A battery of 100 pairs, 5 inches square, operating on nitrate of silver and copper, to produce malachite at the positive pole; at the negative pole, crystals already appear with decided angles and facets.

5. A battery of 16 pairs, of 2 inches, in small glass jars, acting on a weak solution of nitrate of silver, and already producing a compact vegetation of native silver.

6. A battery, esteemed his best, of 813 pairs, 5 inches insulated on glass plates on deal bars, coated with cement, and

so slightly oxydated by water, as to require cleaning but once or twice a year by pumping on them. I felt the effect of 458 pairs in careless order and imperfectly liquidated, and they gave only some tinglings of the fingers, but this power in a few weeks produces decided effects on minerals.

7. A battery of 12 pairs, 25 inches zinc and 36 copper, charged two months before with water, and acting on a solution of nitrate of silver, poured on green-bottle glass coarsely powdered. It had already produced a vegetation of silver.

8. A battery of 129 galley-pots, with semi-circular plates of 1¼ inch radius, placed on glass plates, and acting five months through a small piece of Bridgewater porous brick, on a solution of silex and potash. I saw at the poles small crystals of quartz.

9. A battery of 30 pairs, similar to No. 8. acting since July 27th, on a mixture, in a mortar, of sulphate of lead, of white oxide, of antimony, and sulphate of copper, and green sulphate of iron (205 grains), and three times the whole of green-bottle glass (615 grains). The result has been in five weeks, a precipitation on the negative wire, of pure copper in two days, and crystallized iron pyrites in four days. It had been expected to produce sulphurets of lead, copper, and antimony, by depriving the sulphates of their oxygen.

10. A battery of 5 jars, with plates of different metals, as two copper and platina, one of lead and lead, and one silver and iron, and one copper and lead.—Experimental.

11, 12, and 13. About 200 pairs, in three batteries, working in a dark room, of which I took no note.

While I was an inmate with Mr. Crosse, we had various conversations about the power which he employed. I had in some degree anticipated his debut, by hazarding in the last edition of my "Million of Facts," (1835), and in another work in 1832, an assertion that, inasmuch as metals are found only in a mixed or confused state of different rocks, among which a galvanic action on air or water would necessarily arise, so this in long time would generate the compound matrices of metals; but I did not regard this anticipation as any interference with his original merits, and I was deeply penetrated by the view of his labours, and the expence and zeal with which he had prosecuted his experiments. Yet he had a round conductor for a minimum of power, instead of a combination of flat or parallel ones for a maximum. And he could not help talking about *the fluid*, and some other fancies of the elder electricians, who invented their doctrines before it was suspected that air was a compound, and that such active powers as oxygen, nitrogen, hydrogen, and their definite numerical

co-mixtures, conferred mechanical character on the most refined operations of nature.

He instructed me in the fact that his batteries performed four times the duty in those hours in the morning, from 7 to 11, when the great laboratory of nature is evolving the most oxygen, than in the same period in the evening, when we may imagine the contrary effect takes place. He considered the air as so non-electric in damp weather, that no plate of air, lying between the coatings of a cloud and the earth, could then be disturbed: and he stated to me as a general fact, that the earth is always positively electrified. It also appeared that light interfered with some of his results, for some experiments and conversions succeeded in a dark room which failed in a light one.

On my part I enlarged to him and his son on the universality of matter and motion in producing all material phenomena, independently of the whimsical powers invented in ages when he would have been burnt for a magician; and in this way I endeavoured to return the various information which he had so unreservedly imparted to me. I impressed on him that all this creative energy of atoms was merely a display of developments of the great motions of the earth as they affect the excitable parts of different solid bodies; the results of which are necessarily regular, and their ultimate laws of re-action and combination also regular, so as to produce that universal harmony which surprises beings who, in eternal time, live and observe within only a unit of time. Hence that terrestrial galvanism arising from the operations of the internal frictions and varied pressures called heat; hence those factitious productions of metallic matrices, and crystalline forms, resulting from refined and subtle actions, which confer electrical and galvanic effects, where different substances are proximately opposed; hence magnetism itself tangentially displayed as a resultant of terrestrial currents of electricity; hence, in fine, the wisdom displayed by Mr. Crosse, in resorting to the *modus operandi* of nature in his attempts to imitate her most curious productions.

Observing that continual fresh arrivals rendered it ineligible for me to prolong my visit, I proceeded to Taunton, a distance of six or seven miles, the nearest place at which a stranger can meet with public accommodation.

Here ended my trip to Broomfield: but it is impossible to quit the subject without reflecting on the new career of science which is opened by these experiments. We know that no metals are traced in primitive formations, but oxides of iron and manganese; and that all other metals are found in *subse-*

quent formations, where they appear as accidents of a compound mixture of different rocks in their veins and fissures. Then, as all electrical action is created by the varied excitement of different surfaces in correlative action and re-action, by which the elements in the space are so separated as to carry from surface to surface the *aura* of the opposed plates, and act on effluvia ascending in the vein in combination with the elements; so in this fit arrangement, acting in long time, we see the means by which crystals and all the compound gangues and matrices of metals may be formed.

As the operations of the effect called electric, &c. so intensely occupy the experimental world, I cannot resist this opportunity to add my incidental contribution. I have always referred these atomic actions and re-actions to oxygen and hydrogen, for we encounter these elements in ninety-nine experiments in one hundred. It is, however, necessary to have due regard to their peculiar excitement in all electricity: for though the united poles of a great battery, and the oxy-hydrogen blow pipe, indicate the action of the same elements and produce the same phenomena, yet in one we have the uncontrollable excitement called electricity, and in the other two perfectly manageable gases.

It is then this difference which is to be considered. Why should not the electrical oxygen and hydrogen be like the two gases so called when evolved from manganese and by zinc. To understand this, we have to go one step backward. In space, the gases are so mingled as *by their combined actions to fill every equal space with equal power.* This universal power, therefore, governs in fit co-mixture the union of the elements with *a force* not yet appreciated. We then by our electrical excitements separate one of the components in the space, and we contrive to set bounds to its re-union with the other element previously in fit combination with it, the two having served to fill the space with their power. We have in fact separated two natural allies; and it then is their *endeavour* to re-unite with the power of space; and this effort and their *actual* restoration constitute all the phenomena of electricity. It renders the re-union of poles and of the blow pipe so similar in display, but in the mechanical action of the tools so different. The one is oxygen and hydrogen in *electrical tension;* i. e. seeking re-union with the power of space, and the other oxygen and hydrogen in a couple of bladders!

Till we had decomposed air and water and determined the existence of their elements, we were unable to improve on the invention of the Will-o-the-wisp fluids, and the subtle agents put forward by the early electricians, and maintained by dis-

graceful abuses of mathematics. Many mystics of our own day are, however, so partial to mysteries, that the generation must, I fear, be changed before our understandings cease to be offended with the subtle spirits of Albertus Magnus, and Paracelsus.

It is high time, too, that in all cases we regard electrical action as existing only in *electrics;* and as re-acted upon and *stopt* by the surfaces so incongruously called conductors. These set bounds to a disturbed stratum, and extend or spread the effect only because, if perfect, they partake of none of the action.

Under the delusion of these radical mistakes, it is really surprising that we have blundered into so many amusing results; but I venture to say that if pride will permit us to reason rightly, all the mysteries and freaks of electricity will be understood and practically applied in a few years to many important and unsuspected purposes.

Mistakes about the agents and their nature have prevented our discriminating what results from different re-agents. These are different in machine, voltaic, and magnetic excitements, and the results necessarily correspond with the structure of each. We do not get showy displays from Voltaism generated in interstices of an inch, and refined and exhibited as it is by the difference between two conductors; and we get even less in the diminutive excitements promoted by the scaly structure of iron. The principle is the same, but the re-agents afford different results; and, perhaps, the same principle operates in as a great variety as there are sets of re-agents.

The similarity is evident in the polar character of each, and that the action is in the space without the miscalled conductors is evident from the inductive action of each; while it is palpable that the induction accords with the display, and hence a magnet acts only on iron, because similarity of excitement permits only similarity of action, for which iron by structure alone is adapted.

This theory is what I have laboured to inculcate for at least forty years; and if I am desirous of reiterating it now, it is because I heard all the authorities at Bristol declare publicly that the new facts could not be explained by any of the established theories. Of course it is a subject on which I might enlarge, might adduce proofs, answer objections, &c.; but truth will prevail, and those who are practised in these sciences will readily confer fulness on my brevity.

The analogy between the inductive powers of voltaic wires to produce magnetism, and of terrestrial electricity to produce directive polarity, is the finest idea of this age, if it can

be maintained. It well accords with the friction and pressure of the earth's two motions, so calculated to generate universal electrical action; but the dip demands that the currents should be at great depths, and this and some other anomalies require to be reconciled. I have some doubts, but I wish it success for its magnificence.

R. P.

P.S.—To Mr. Sturgeon.—I have just received a letter from Mr. Crosse, of the date of November 30th, in which it appears that the late hurricane has destroyed thirty-five of those fine trees which adorn his grounds; one of them a beech, containing nine tons of timber, 100 feet high, and spreading over 550 square yards. He reports that he is busily engaged in an infinity of new arrangements; but he states that experimentalists have mighty difficulties to grapple with, though the most important results will, sooner or later, be brought to light. In the first place, he observes, we want a good analysis of the voltaic battery, with a proper discrimination between its intensity and density, so as to determine the cause of simple electric action, the decomposition of fluids, and the fusion of metals; and an explanation of its union with magnetism, light, heat, &c. comprehending the different quantities in each of the solar rays. We next want a correct account of the natural electric action and re-action of simple and compound substances, beginning with one that is positive to every other, and ending with one that is negative to every other. If, says Mr. Crosse, an experimentalist could be armed with a tolerable share of this knowledge, he might be expected, pretty quickly, to bring to light many results worth knowing.

In reply to some suggestions of mine about the possibility of obtaining a permanent light and heat for all domestic purposes, by voltaic action, without the use or consumption of any combustible substance, he observes that he does not think it absolutely impossible; but that, in the present state of the science, it would be difficult to point out a mode.

R. P.

XXVII. *A Description of a Magnetic Electrical Machine, invented by* E. M. Clarke, *Magnetician, of Agar-street, Strand.*

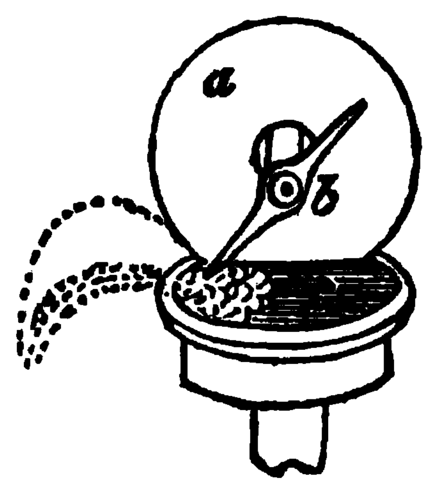
This apparatus, which, with the exception of there being *rotating armatures* and a *magnetic battery,* differs from any magnetic machine which has hitherto been constructed.

The October number of the *Philosophical Magazine* for 1836 contains a brief account of this machine; it being the intention of the inventor to reserve a more detailed description for insertion in the "*Annals*," in consequence of its being the aim of the Editor of the latter named periodical to make this deservedly interesting branch of science one of the leading features of the work. Since that time, a most important improvement has been made, by the rejection of the mercury box. By the inventor's present arrangement, the necessity of using mercury is superseded. Fig. 1.

A, is the battery of bent bar magnets, placed *vertically*, and resting against four adjusting screws, which pass through

the mahogany back board B (two of them are shown at M, N. fig. 12). C is a bar of stout brass, having an opening in the middle, through which passes a bolt with a screw-wheel, the purpose of which is to draw the magnetic battery to the board B. By these means, the battery can readily be disengaged from the machine, without taking asunder the entire apparatus, and the battery is thus also freed from that vibration which must necessarily be occasioned by the attachment of the rotating apparatus to the battery itself. D is the intensity armature, which screws into a brass mandril seated between the poles of the battery A; motion being communicated to it by the multiplying wheel E. This armature has two coils of fine insulated copper wire, 1500 yards long, coiled on its cylinders, the commencement of each coil being soldered to the armature D, from which projects a brass stem, (also soldered into D), which carries the break-pieces, H and H. The break-piece is made fast in what position soever is required by a small binding screw. K and K a hollow brass cylinder, to which the terminations of the coils F G are soldered, being insulated by a piece of hard wood attached to the brass stem. O and O are iron wire springs, pressing against the hollow cylinder K at one end, and held in metallic contact by a nurled head-screw in the brass strap M, which is fixed to the side of the wooden block L. P and P a square brass pillar, fitting into a square opening in the other brass strap N, and secured at any convenient height required. Q and Q, a metal spring that rubs gently on the break-piece H, and held in perfect metallic contact by the nurled head screw in P. T and T a piece of copper wire for connecting the two brass straps, M, N.; then D, H, Q, P, N are in connexion with the commencements of each coil, and K, O, M with the terminations. The advantages of this arrangement must be obvious to any person who has seen the magnetic machine in action in the Adelaide Gallery of Practical Science, where the old arrangement of the mercury flood is still used, where both disc and blades scatter the mercury about as in fig. 1': *a* the disc, *b*, the double blades, *c*, the mercury flood. The loss of mercury is not the only evil; for as you continue working the machine, you of course lose the adjustment you had at starting, and the effect is constantly diminishing, and will at length cease, owing to the points *b* not having mercury to dip in. By the new arrangement, the metal spring Q presses gently on the break H; consequently, the effects here are unbroken, no matter how long you

FIG. I.



may require to keep the machine acting. This is not the only advantage it possesses; for in the mercury the surface is very rapidly oxidated; the oxide adheres to both disc and point, and preventing so perfect a metallic contact as that obtained by the spring and break.

To adjust the intensity armature.

See that the faces of the iron cylinders that carry the coils F, G, fig. 1, are parallel to, and all but in contact with, the magnetic battery A; if not, unscrew the nut of the multiplying wheel E, and take it off its axis: you then have at your command the four screws against which the battery rests, (two of which are to be seen at M N, fig. 12); by means of them and the nut of the strap C, you can adjust the battery to the greatest nicety. The next step is to adjust the break, so that the spring Q will separate from it just at the same time that the iron cylinders of the armature have left the poles of the magnetic battery; and lastly, see that the iron wire spring O, presses gently against the hollow brass cylinder K.

To give the Shock.

Grasp the two brass conductors, R S, in the hands,* put one of their connecting wires into the holes of either of the brass slips M or N, the other wire into the hole at the end of the brass stem that carries the break H. Connect M N by T, turn the multiplying wheel in the direction of the arrow, and a violent shock will be received by the person holding R S. The shock which is obtained from the intensity armature having 1500 yards of fine insulated wire, is such that a person, even of the strongest nerves, will not readily volunteer to receive a second shock. Indeed the effects are so violent, that the inventor has proposed to many of his military customers that this instrument would be a good substitute for the lash, being capable of producing even greater torture than that brutal instrument, without producing any corporeal injury to the delinquent. Place R S in two separate basins of salt and water, immerse a hand in each basin, and the shock will also be felt very powerfully; this method is to be preferred, as it leaves the person who is electrified the power of quitting when he pleases; not so with the conductors; for the muscles of the arms contract violently, so as to close the hands completely on the conductors, taking away the power of letting them go. If the two connecting wires of R S are put in M N, the shock is not so powerful. The shock can be modified in different ways. By turning the wheel E very slowly,

* If the hands are wetted with vinegar or salt and water, the effect is considerably increased.

or increasing the distance between the battery A and the armature D, or by making the break H separate from the spring Q when the armature D is horizontal. U V a pair of directors, holding a piece of sponge, each to be used when the electricity is to be applied medicinally. The connecting wires are to be placed in the same way as the conductors are in the figure; the sponges must be wetted with a little vinegar or salt and water, so as to make them conduct the electricity. By those directors a succession of most powerful shocks can be given, when the case requires it; or they can be so modified as to be barely felt by the most nervous patient.* Remove T from M N, put the two pieces of iron wire with an end of each in its place; put other ends of them into two holes that are in the sides of the battery A; let the wires be sufficiently long to allow the armature to rotate between them. If one wetted finger is placed on the brass stem that carries the break H, and another wetted finger is placed on the magnetic battery, the shock will be also felt. While the machine is so arranged, if you look between the face of the rotating armature and the magnetic battery, vivid flashes of light will be perceived playing between both. This light may also be frequently seen without the wires being in connexion with the battery. Sometimes it will be observed flashing between the coils F G.

* To medical gentlemen, the instrument may be strongly recommended from the following advantages, Its portability; its being always in a fit state for action, even in the dampest weather; the nicety with which the power of the shocks may be increased or diminished. Indeed it combines the advantages of the electrical machine and the galvanic apparatus; at the same time that it does not labour under the disadvantage of either; for, as has already been stated, it is not affected, like the former, by a moist condition of the atmosphere, nor, like the latter, is it necessary to make use of any acids; nay, since the improvement has been effected which is alluded to in the text, even the use of mercury is superseded.

To decompose Water, &c. &c.

Fig. 2. E. M. Clarke's arrangement of the decomposition of water apparatus, (See Phil. Mag. for June, 1835.) A, a glass vessel, having a brass cap with hardwood bottom, through which two pieces of copper wire pass, having pieces of platina wire soldered to them; place this in M, N; fill the tube B, with water,* thrust it over the platina wires where it will be held by the cork C. Q must rub on the cylindrical part of the break H. Here the gases are obtained mixed.

Fig. 3. E. M. Clarke's arrangement of the apparatus for obtaining the elements of water in separate vessels, or unmixed. A, a glass vessel having two glass tubes; here the platina wires are soldered to two pieces of copper wire, as in my other arrangement, but differing inasmuch as that they are also soldered to the two brass cups b b, which are intended to hold a little mercury. Connect it by copper wire; a little acid or any salt will increase the effect by forming a better conductor with M, N, as in

* The advantages of this arrangement are obvious to any one who has been teased with bits of platina wires made to pass through small holes drilled in a glass vessel having loops turned on the projecting ends, and contact is obtained by merely placing the connecting wire in the loop: it was not only a bad connexion, but 9 cases out of 10 the cement that is used to fasten in the platina wires gave way, just as you were going to use the apparatus, as has frequently happened at lectures.

the figure. Here Q must work on the single break, H. C, D, two platina plates, having two copper wires soldered to them to connect them with M, N; on placing a piece of litmus or turmeric paper between them that has been previously wetted with some neutral salt, its decomposition is shown by the alteration in the colour of the paper. You may even transpose the colours by altering the position of the break H.

Description of the quantity Armature.

This armatnre differs materially from that which is employed for intensity. The latter, as already stated, has two coils of 1500 yards of fine insulated copper wire. The inventor has also tried silver wire, which he found to be superior to copper in the proportion of nearly 3 to 1. The quantity of iron in the cylinders also is much smaller than in the quantity armature, whose effects are greatest when the quantity of iron in the cylinders is increased, and the length of the copper wires diminished; the wire at the same time for quantity being much thicker. The quantity armature contains only 40 yards of wire.

To adjust the quantity armature and exhibit the spark.

Fig. 4. A, the magnetic battery, D, the armature, and F G, two coils of copper wire containing 20 yards each. Care must be taken that the spring separate from the break at the same time that the armature is vertical, being then in a neutral position as respects the poles of the battery.

To scintillate iron wire.

Connect one end of a piece of iron wire with P, fig. 5, pressing the other end gently on the surface of the rotating armature, and brilliant scintillations will be obtained. This effect cannot be produced by any magnetic machine unless it be constructed similarly to E. M. Clarke's;

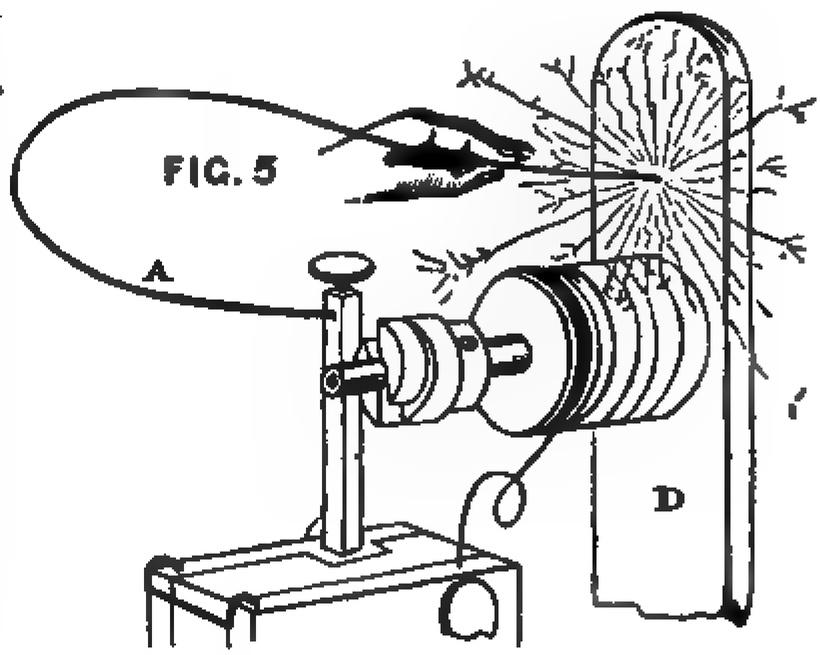


FIG. 5

the effect depending upon the wires being *soldered* to the armature; whereas, in other armatures the wire is insulated throughout.

To make platina wire red hot.

Fig. 6 shows the arrangement for this purpose, A being placed in contact with P and H. Whilst the platina wire is red hot, ether may be inflamed, gunpowder exploded, and other experiments of a similar nature be performed.

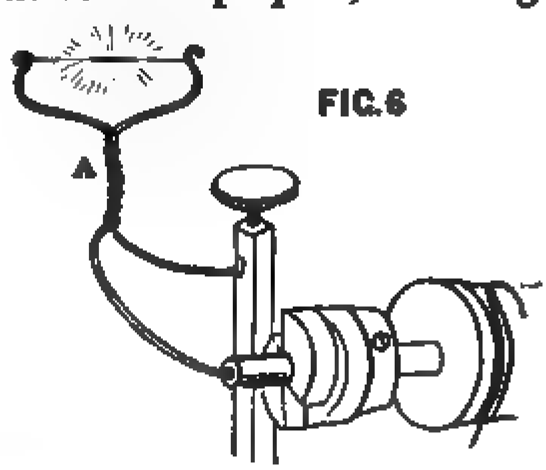


FIG. 6

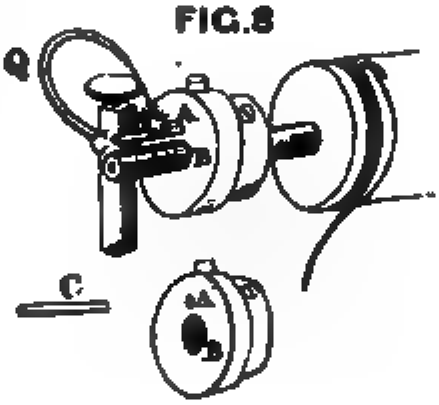
To render soft iron magnetic.

Fig. 7. A, a piece of iron bent as in the figure. B. a soft iron keeper, which adheres to the iron on the connexion being made as represented, so long as the machine is in action.

To obtain sparks of various colours by the use of different metals.

Fig. 8. Remove the break, and substitute the brass piece B. Into the small hole insert a piece of wire C, of any metal, for instance gold. Let the extremity of the spring Q be also of gold. On rotating the machine, sparks of a purple colour will be obtained.

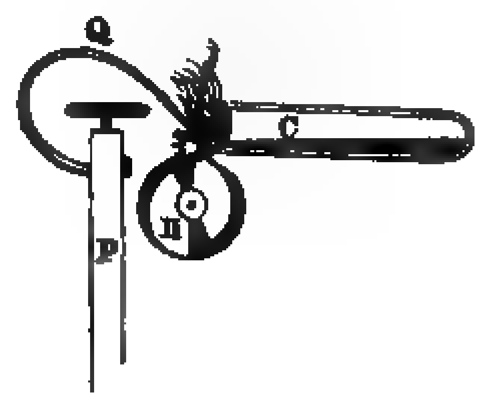
FIG.8



To exemplify the disadvantages attending the mercury flood.

Fig. 9. Remove the break, and fix the double-blades B, in its place. Adjust the brass cup A so that the point will leave the surface of the mercury when the armature is vertical. The brilliancy of the spark, as thus obtained, appears much greater than it is in reality, the additional brightness being occasioned by reflection from the surface of the mercury. It is almost unnecessary to point out the evil that arises from the scattering of the mercury, not only in point of cleanliness, but expense. A little ether, spirits of wine, or naphtha being poured on the mercury, is readily inflamed. The same experiment may be satisfactorily performed, by pouring any of those liquids into a test tube C, and holding it over the break. The vapour will speedily be ignited by the magnetic spark.

FIG 9

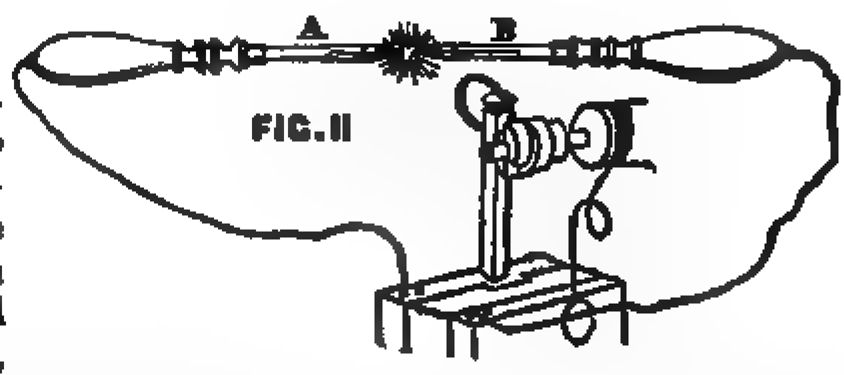


To produce rotation by magnetic electricity.

Fig. 10, A, a vertical horse-shoe magnet, on a tripod stand B; C, improved flood-cups; D, the wire frames, having two little cups at top to hold a drop of mercury; E, a connecting fork. Mercury being poured into the flood cups C, and the single break X being used, on placing the connecting wires as in the figure, continuous rotatory motion will be produced by this arrangement, the current being constantly in the same direction. But the experiment may be varied by substituting the double break H, (fig. 1), the currents now alternating.*

To ignite Charcoal.

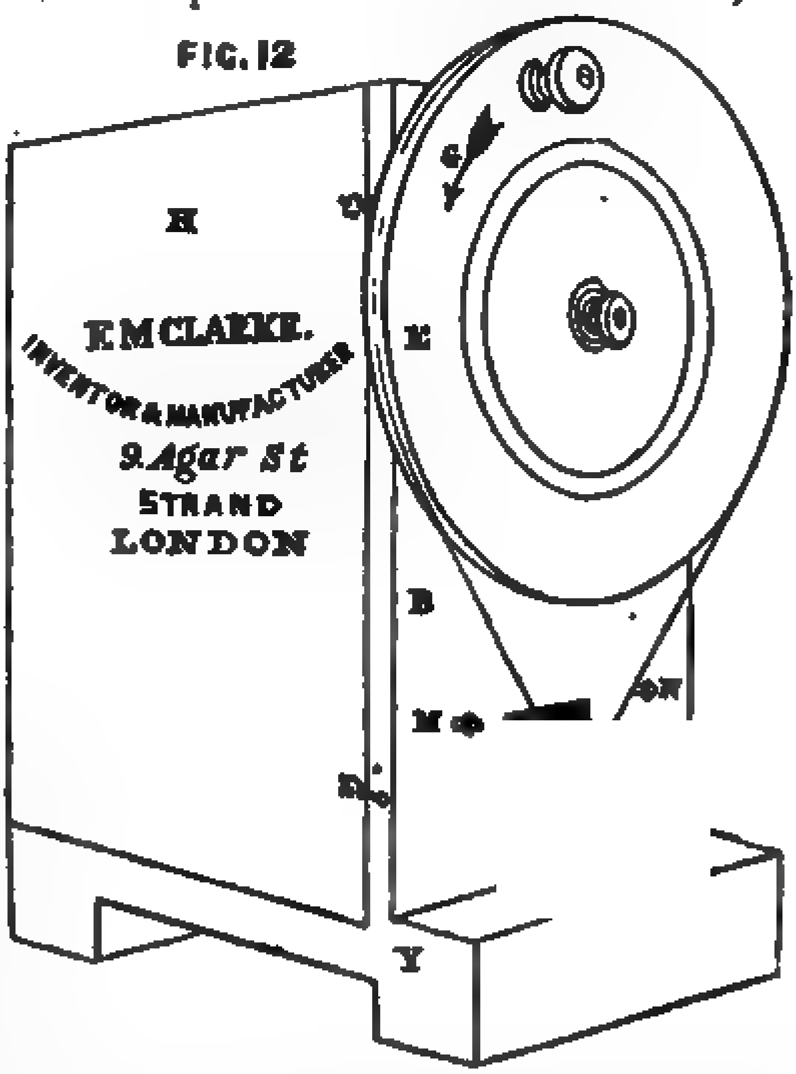
Fig. 11 represents the arrangement of the apparatus for this purpose. The same directors that are used to hold the sponges, may be used to retain the charcoal points A, B, in the proper position.



* A singular fact connected with this experiment is the rotation of the two wire frames in the same direction, owing to passages of the electricity from one of the wire frames into one pole of the magnet, and then from the other pole of the magnet down the other frame.

Fig. 12, shows the compactness of the machine. H, a mahogany case sliding on the bottom board Y, which locks against the back board B. The multiplying wheel is to be turned in the direction of the arrow G. D, the pulley, and C the mandril that carries the armatures.

FIG. 12



E. M. Clarke on the occasion of his last visit to Paris, had the honour to exhibit the effects of the magnetic machine which forms the subject of the present paper, to several of the French Savans, all of whom were pleased to express their unqualified approbation. M. le Baron, Léguier, brought the inventor to the French Institute, accompanied by M. Chevalier. Amongst others present, during the exhibition of the machine, were MM. Melloni, Dulong, Savary, Becquerel, and others. Professor Arago, who was that day officially engaged, having heard the result of the experiments with the machine, requested the inventor to attend the day following at the Observatory, which he did; and that learned Professor also expressed his satisfaction. On the day following, in consequence of a note received from M. Pouillet, he attended at the Conservatory of Arts and Sciences, when that learned Professor, who, of course, was well acquainted with the previous magnetic machines, as Pixii's, Newman's, (the name by which Saxton's machine is known on the Continent), &c. gave the decided preference to E. M. Clarke's arrangement; in proof of which, he was pleased to direct that one should be constructed for the Conservatory of Arts, and another to be deposited in the cabinet of his royal pupil, the Duke of Orleans.

XXVIII. *A letter to the Editor of the Annals of Electricity, on a new Experiment.*

Sir,

If the following appear to you to be worthy of a place in your pages, I beg you will give it insertion.—

In constructing the sustentatory battery of Professor Daniell, in which intestinal membrane or bladder is made to separate the two metals, copper and zinc, with their proper fluids, I was struck with the very surprising permeability of the membrane to the Voltaic influence, whilst it as obstinately resisted all communication between the two fluids, or any interference of their chemical qualities, be they ever so dissimilar, or ready to neutralize each other, as in the case of muriate of ammonia and sulphate of copper.

Should we not rather seek the solution of this problem in some quality in the gut or bladder, and consider it consequent upon some law in physiology, and neither chemical nor electric?* The experiment I lately made tends much to bias me in that belief: but whatever way it may be reasoned upon, being confirmatory of a curious fact, I think it deserves to be recorded.

I took a smooth and clean piece of zinc about the size of a walnut, inserted it in a piece of bullock's gut, and having carefully squeezed out all the air from the gut, tied it tight at both ends. I then placed the zinc so surrounded by the gut (into which there could be no mechanical possibility of entrance to the fluid) into a jar of dilute sulphuric acid, and left it so all night. In the morning I found the gut full of gas, almost to bursting. To prove the nature of the gas I made a pin hole in the membrane, and, squeezing the gas through the flame of a candle, found, as I expected, that it was hydrogen.

Here there could have been no Voltaic influence; for zinc

* That animal membrane (bladder gut) has not only a permeability to fluid, but a power by capillary attraction, of raising it two or three feet above its level. I call to witness my friend Goldworthy Gurney, who lately related to me the following experiment: fill a glass funnel with water, and tie a fresh bladder over the large opening so securely that no water can escape Place it with the bladder downwards in a vessel (large dish) also full of water. Into the nozzle or small end of the funnel a tube of 3 feet in length is to be inserted and secured quite tight by luting; set the apparatus thus arranged aside and watching it, it will be found, said my informer, that the water will soon begin to rise in the tube, and ultimately reach the height of two or three feet.

alone can produce none.* Chemical it could only have been by the transmission of the acid; but the gut contained none. The zinc bore the same clean appearance as when first inserted and nowise oxidated.

I repeated the experiment again and again with the same result, excepting only that I found the inside of the bladder or gut and zinc slightly moistened with a tasteless vapid fluid.

L. B. W.

To W. Sturgeon, Esq.
&c. &c. &c.

XXIX. *To the Editor of the Annals of Electricity, Magnetism, and Chemistry, &c., &c.*

Sir,

Encouraged by the friendly invitation given in the prospectus to the first number of your much wanted "Annals," in which you also say, "that every description of new experiment or instrument in electricity, &c. shall find a place in your work," I have determined to forward you the following remarks: and should you consider them worthy of a place in your valuable Annals you will oblige me by their insertion. I have been for some time past endeavouring to make myself master of what is already known on the subject of galvanism. My battery consisted of 50 pair of the usual four-inch plates, with double coppers, on Dr. Wollaston's plan, and I could not but feel the great advantage that must be derived, could we conveniently clean and amalgamate the zinc plates.

I am happy to say I have hit on a plan by which we can easily effect this very desirable object, and which for several reasons renders it in my very humble opinion far superior to any other arrangement.

Fig. 38. shows the copper, flattened out. Pieces of curved wood fig. 39, are fixed at the points *a, a,* to prevent contact between the metals. The copper plate is then bent up, and the ends of the copper straps F, F, are soldered to their then opposite sides. The two upright pieces B, B, serve to suspend the whole from the frame work, fig. K, which surrounds the upper part of the trough.

Wooden cups to contain mercury are soldered at C, fig. 38, and are made of the form shown in the figure to prevent the mercury being thrown out by any slight motion of the battery.

The zinc plate is shown at fig. 40, and has a copper wire soldered to it, which is amalgamated at its outer end, and bent

* See our article on the electro-chemical action of fluids on simple metals, commencing at page 11. EDIT.

so that it dips into the mercury cup of the next copper cell. Thus we have perfect metallic contact; and the zincs may at all times most easily be removed, cleaned, and returned to their places. The copper and zinc pair, when complete for the battery, is represented by fig. 41.

The advantages of this arrangement are, I think, too obvious to require much description. The same troughs, zincs, and coppers of the old ones, will do also for mine; and we can also obtain either *quantity* or *intensity*, by placing the troughs, as mentioned by Mr. Faraday in his "Manipulation."

The connexion being more perfect between the plates, an advantage must also be obtained in this respect.

And now allow me to mention another little matter.

About four months ago in reading Dr. Faraday's Experimental Researches, in the Phil. Trans., article 1049, I observed the following remark: "If the wire which surrounds an Electro-magnet be used as the medium of communication between the plates of an electromoter, consisting of a single pair of plates, a shock is felt each time the contact is broken, provided the ends of the wire be grasped one in each hand." Following this up, I thought that by the mechanical means shown in the drawing, I could easily manage to get a quick succession of shocks, and thus produce the effects of a number of plates. Of this I wrote to my friend Mr. Watkins, the celebrated Optician, and in his reply, dated October 13th, he says "pray let me know how you affect the nerves with a single pair of plates," and "tell me when you write if the method originated with yourself, or from whom you got the notion." I have shown in the quotation, from Dr. Faraday's paper, from whence I got the first idea.

I also have read your article on this subject in the "Annals" p. 67, and which is dated Sep. 28. From my own private experiments, and from the above last mentioned printed facts, I was surprised to read in the Philosophical Magazine for this month (Dec.), a paper by the Rev. N. J. Callan, describing what he terms "a new Galvanic Battery." As he does not give any exact description or drawing of the apparatus by which he used to get the shock, it may not be altogether unacceptable to some of your readers, to see the exact method used by myself, and which by the bye I think is rather better than your "rough edged wheel." The above figure will speak for itself.*

* Fig. 42, Plate VI. is that to which Mr. Barker refers, and we think it quite possible that an electro-magnetist might easily understand it; but as it is also possible that some of our readers may be at a loss to comprehend the arrangement and the manner by which the apparatus acts, it would have been more satisfactory if we had been favoured with the necessary description. Mr. Barker will

I may, however, just add, that I do not find the shock so strong when the communication is made and broken too quickly, but this may perhaps be on account of the smallness of the apparatus I have employed.

I am now making a powerful helix of 2000 feet of copper wire round a glass tube three feet in length, in which I can pass a bar of soft iron of the same length, and one inch diameter, and then I can see if we have a powerful shock, by the presence of the iron, or not. Should I arrive at any conclusion worth communicating, you will again hear from,

Your humble Servant,

CHARLES BARKER.

Gosport, Dec. 12th, 1836.

XXX. *On the production of Electric Shocks from a single Voltaic pair.*

Since the publication of the first number of these "ANNALS," I have seen, for the first time, that part of the Philosophical Transactions which contains Dr. Faraday's ninth series of "Experimental Researches &c." and find in that series, several experiments described, by which electric shocks are produced from the action of a single voltaic pair; and other particulars relative to the powers of coils in the conducting circuit, similar to some of those described in my paper commencing at page 67. I regret very much that I was not acquainted with those experiments before my paper was published; for, it must at all times be an unenviable position for any one to be placed in when in search of new facts, not to be acquainted with what had been done before; and more particularly so if he should happen to place in his own collection of discoveries any of those which had previously been made, and which is justly and rightfully the property of others. It appears, however, that I have been led to some experiments which had previously been made by Dr. Faraday, and which have been attended with the same results as those discovered

readily perceive that we are desirous of forwarding his views and of paying every respect to his labours; and we hope that he will favour our readers with an exact description of his ingenious apparatus through the medium of our next number.

We beg to solicit the attention of our other correspondents to the above remarks; and also to inform them that we cannot make any pledge to print drawings of any apparatus unaccompanied by proper description. EDIT.

by that gentleman. So far, I take much pleasure in conceding what I have done; but there are other experiments detailed in my paper, which have no bearing on Dr. Faraday's enquiries: and the views which we have taken of the nature of the action will appear perfectly distinct from each other. With regard to the difference of our results when using iron in the coils, I suspect it may probably be owing partly to the different fashions of our coils, and partly to the difference in the powers of the batteries employed. Dr. Faraday employed long narrow coils: whilst those which I employed were the short thick ones belonging to a magnetic-electrical machine: and the iron I employed was the revolving armature belonging to them.

I have not yet had time to repeat the experiments, but mean to do so shortly; and publish the results in the next number of these Annals: and in order to give that degree of credit to Dr. Faraday to which his experiments entitle him, I take much pleasure, at this earliest opportunity, in placing them before the readers of the "Annals of Electricity, &c." in his own words.

W. Sturgeon.

III. *Experimental Researches in Electricity.—Ninth Series.* By Michael Faraday, *D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.*

Received December 18, 1834. Read January 29, 1835.

§. 15. *On the influence by induction of an Electric Current on itself:—and on the inductive action of Electric Currents generally.*

1048. The following investigations relate to a very remarkable inductive action of electric currents. or of the different parts of the same current, and indicate an immediate connexion between such inductive action and the direct transmission of electricity through conducting bodies, or even that exhibited in the form of a spark.

1049. The inquiry arose out of a fact communicated to me by Mr. Jenkin, which is as follows. If an ordinary wire of short length be used as the medium of communication between the two plates of an electromotor consisting of a single pair of metals, no management will enable the experimenter to obtain an electric shock from this wire; but if the wire which surrounds an electro-magnet be used, a shock is felt each time the contact with the electromotor is broken, provided the ends of the wire be grasped one in each hand.

1050, Another effect is observed at the same time, which has long been known to philosophers, namely, that a bright electric spark occurs at the place of disjunction.

1051. A brief account of these results, with some of a corresponding character which I had observed in using long wires, was published in the Philosophical Magazine for 1834*; and I added to them some observations on their nature. Further investigations led me to perceive the inaccuracy of my first notions, and ended in identifying these effeets with the phenomena of induction which I had been fortunate enough to develope in the First Series of these Experimental Researches†. Notwithstanding this identity, the extension and the peculiarity of the views respecting electric currents which the results supply, lead me to believe that they will be found worthy of the attention of the Royal Society.

1052. The *electromotor* used consisted of a cylinder of zinc introduced between the two parts of a double cylinder of copper, and preserved from metallic contact in the usual way by corks. The zinc cylinder was eight inches high and four inches in diameter. Both it and the copper cylinder were supplied with stiff wires, surmounted by cups containing mercury; and it was at these cups that the contacts of wires, helices, or electro-magnets, used to complete the circuit, were made or broken, These cups I will call G and E throughout the rest of this paper (1079).

1053. Certain *helices* were constructed, some of which it will be necessary to describe. A pasteboard tube had four copper wires, one twenty-fourth of an inch in thickness, wound round it, each forming a helix in the same direction from end to end: the convolutions of each wire were separated by string, and the superposed helices prevented from touching by intervening calico. The lengths of the wires forming the helices were 48, 49·5, 48, and 45 feet. The first and third wires were united together so as to form one consistent helix of 96 feet in length: and the second and fourth wires were similarly united to form a second helix, closely interwoven with the first, and 94·5 feet in length. These helices may be distinguished by the numbers i and ii. They were carefully examined by a powerful current of electricity and a galvanometer, and found to have no communication with each other.

1054. Another helix was constructed upon a similar pasteboard tube, two lengths of the same copper wire being used, each forty-six feet long. These were united into one consist-

* Vol. v. p. 349.

† Philosophical Transactions, 1832, p. 126.

ent helix of ninety-two feet, which therefore was nearly equal in value to either of the former helices, but was not in close inductive association with them. It may be distinguished by the number iii.

1055. A fourth helix was constructed of very thick copper wire, being one-fifth of an inch in diameter; the length of wire used was seventy-nine feet, independently of the straight terminal portions.

1056. The principal *electro-magnet* employed consisted of a cylindrical bar of soft iron twenty-five inches long, and one inch and three quarters in diameter, bent into a ring, so that the ends nearly touched, and surrounded by three coils of thick copper wire, the similar ends of which were fastened together; then each of these terminations was soldered to a copper rod, serving as a conducting continuation of the wire. Hence any electric current sent through the rods was divided in the helices surrounding the ring, into three parts, all of which, however, moved in the same direction. The three wires may therefore be considered as representing one wire, of thrice the thickness of the wire really used.

1057. Other electro-magnets could be made at pleasure by introducing a soft iron rod into any of the helices described (1058, &c.).

1058. The *galvanometer* which I had occasion to use was rough in its construction, having but one magnetic needle, and not at all delicate in its indications.

1059. The effects to be considered *depend on the conductor* employed to complete the communication between the zinc and copper plates of the electromotor; and I shall have to consider this conductor under four different forms: as the helix of an electro-magnet (1056.); as an ordinary helix (1053. &c.); as a *long* extended wire, having its course such that the parts can exert no mutual influence; and as a *short* wire. In all cases the conductor was of copper.

1060. The effects are best shown by the *electro-magnet* (1056.). When it was used to complete the communication at the electromotor, there was no sensible spark on *making* contact, but on *breaking* contact there was a very large and bright spark, with considerable combustion of the mercury. Then, again, with respect to the shock: if the hands were moistened in salt and water, and good contact between them and the wires retained, no shock could be felt upon *making* contact at the electromotor, but a powerful one on *breaking* contact.

(To be continued.)

XXXI. *Description of a new Rubber for a Cylindrical Machine, by* J. HARPER, Esq. *in a letter to* Mr. Sturgeon.

My Dear Sir,

From seeing you use tin foil to your rubbers of Electrical Machines in order to conduct more effectually a supply of electricity to the cylinder, it suggested to me the idea of a new rubber which I now send you a description of, and if you think it worthy of a place in your Annals, you can insert it in that new and useful periodical.

It consists of a cylindrical piece of metal (I have used rolled zinc) nearly the length of the cylinder for which it is required, and about one inch diameter, open at each end and also in the side about a quarter of its diameter: in one edge of the opening is drilled a row of holes, by which is shown a piece of black silk to form the usual flap: a cushion is made of basil leather of a cylindrical form, square at each end to fit exactly the metal tube, and slipped into it: the open side of the tube now admits the cushion to project so as to form a soft rubber when pressed against the Electrical cylinder: a piece of box wood is screwed at the back of the metal tube with either a tenon or dovetail, to fit to the pillar that supports the rubber; a narrow slip of leather is glued on the silk just where it comes in contact with the glass cylinder on which is spread the amalgam.

I have found this cushion increase the action of my electrical machine considerably, and doubt not you will on trial concur in what I have stated.

When the cushion gets flattened by use, by bringing a fresh part to bear on the glass cylinder, you renew it every time it is moved; and the cushion being square at the ends, a larger portion presses on the machine.

I am,
Your obedient servant,
J. HARPER.

Oxford, 12th Dec. 1836.

XXXII. MISCELLANEOUS ARTICLES.

Extract from a Paper by MR. HAWKINS, *read at the Mantellian Museum.*

In aid of humanity, Electricity is now employed for the prevervation of life. The blasting of rocks in mining districts is effected by inserting tubes of metal containing gunpowder, in the rock, and ignition is produced by a slow match; this

occasionally fails or is retarded in its effect, and the poor miner returning to rearrange it, has, not unfrequently, fallen a sacrifice to his incautiousness. To these tubes Professor Hare, of Philadelphia, has adapted wires from a battery, so that by the electric spark explosion takes place with certainty, while the operator is sufficiently distant to prevent danger.

To chemistry there can be but little doubt this agent will become of infinitely greater importance even than it has hitherto been, particularly for synthetical and manufacturing purposes. The simple fact, hypothetically stated by Davy, but decided by Dr. Faraday and Mr. Sturgeon, that the chemical affinity of unlike bodies is merely the electrical attraction of their primary molecules, at once informs us that chemistry has electricity for its basis, and therefore it might also be expected that the chemist will soon be found to employ the voltaic battery as his most common test. The luminous and heating effects of galvanism are so surprising that although not a recent discovery some of them deserve a retrospective notice. By the famed battery of Mr. Children not less than five feet and a half of stout platina wire was raised to a red heat visible in daylight; this wire if passed through water would boil it, and keep it in continual ebullition for an indefinite period. When the poles of such a battery as that by which Sir Humphrey Davy made his brilliant discoveries of the alcaline metals, terminate in charcoal points, and these brought within a certain distance of each other, an arch of the most intense light occurs between them. Mr. Wm. Allen, my honoured friend, and one of the most respected of men, in one of his lectures at Guy's Hospital, exhibited the power of this light in illuminating objects for the solar microscope; the heat emitted from it is of the highest degree attainable by the art of man, fusing readily those substances most refractory in our best constructed furnaces, such as platina, quartz, the sapphire, magnesia, and lime; and fragments of the diamond appear to be rapidly converted into vapour.

These are only a few of this class of electrical effects, but when Volta's apparatus shall be made to display all its energies, and this instrument of wealth and power to future ages, shall have arrived at its utmost simplicity, to how many purposes of utility will it be applied! As an economical generator of heat it may prove an advantageous substitute for many of the commonest uses of fire, and if, by its decomposition of water into the constituents of that most singular fluid, the gases oxygen and hydrogen be obtained almost without cost, then may the lime light be adopted to illuminate our streets.—*Brighton Herald.*

Aurora Borealis.—11*th and* 18*th Oct.* 1836.

Shortly after 8 o'clock on Tuesday evening, the Metropolis and its suburbs, for miles round, was thrown into a state of the greatest excitement by the northern hemisphere assuming a most awful fiery appearance, which seemed to indicate the existence of some dreadful conflagration in the north portion of the Metropolis. The principal thoroughfares leading to that point were speedily crowded by thousands of persons hurrying to the supposed scene of destruction, whilst the various engine stations were literally besieged by persons in cabs, &c. possessing property in that direction, anxious to ascertain whether or not it was secure from danger. Many of the fire-engines were put into motion, and after scouring the northern parts of the City and suburbs in search of the supposed fire, it was at last discovered that the appearance of the sky had been caused by one of the most splendid of those remarkable phenomena, known as the *Aurora Borealis*, or Northern Lights.

From persons who had the opportunity of witnessing the magnificent spectacle from elevated situations, we understand there first appeared a large luminous arch, extending nearly from north to south, from which streamers appeared, very low, running to and fro from N.E. to S.W. and momentarily increasing in number until they began to approach the zenith, apparently with an accelerated velocity. Suddenly the whole hemisphere was covered with them, when the intensity of the light, the prodigious number and the volatility of the beams, the grand intermixture of all the prismatic colours in their utmost splendour, variegating the glowing canopy with the most luxuriant and enchanting scenery, afforded an awful, but at the same time the most pleasing and sublime spectacle in nature. The splendid scene, however, only lasted about forty seconds; the variety of colours disappeared, and the beams lost their lateral motion, and were converted as usual into the flashing radiations, which kept diminishing in splendour until the whole disappeared, leaving only a pale white light near the horizon. During the aurora, which lasted about half-an-hour, the light of the stars was not respected, numbers being occasionally seen through the luminous arch or beam.

When the northern lights are visible in this country, they appear chiefly in the spring and autumn, and usually after a period of dry weather. They are seen more rarely in countries near the equator; but occur almost incessantly during the long winters in the Polar Regions, and with a lustre of which we can form but a faint conception.—*Times.*

Of late years it has been remarked that the appearances of the Aurora Borealis have been more frequent and more brilliant than they used to be. We have been accustomed to see the Northern Lights playing in fitful flashes across the sky, or shooting up their beautiful and ever-varying silvery streamers from the horizon to the zenith, in the most fantastic forms. On Tuesday night, however, the appearance of this phenomenon was singular; and, as far as we know, altogether new. The wind had blown a perfect hurricane during the day, but became more moderate towards the evening, when the sky cleared, and the moon shone in almost cloudless majesty. A little before eight o'clock the aurora suddenly appeared; at first like a long belt or rope of light, arching the heavens from nearly east to west. It then began to radiate from the zenith towards the horizon in every direction: but the most extraordinary phenomenon was the culminating of the light towards one centre, which appeared bright over the City, resembling somewhat an umbrella half furled. In the middle was a large space, sometimes dark, and sometimes streaked with beams of light, while from the circumference the corruscations shot downwards towards the horizon, diverging on all sides until they melted away in the cerulean hue of the firmament. The magnificent and novel aspect of this lurid canopy struck the spectator with feelings of awe and wonder. But the most remarkable feature of this aurora was the deep crimson colour which it occasionally assumed, and which was perfectly transparent, as the stars were seen distinctly shining through it. This coloured appearance which might easily have been mistaken for the reflection of some conflagration, or part of the city on fire, continued with slight intermissions for nearly an hour, commencing in the east and gradually dying away in the west. The phenomenon, we believe, was quite new in this quarter, and, as might be expected, attracted numerous groups of spectators.—*Edinburgh Advertizer.*

In the years 1715 and 1745 remarkable exhibitions of this meteor made their appearance: these at the time were connected with the rebellions ocurring just afterwards. On the 1st and again on the 7th of January, 1830, similar extraordinary appearances were again witnessed in the sky. These seemed also to be associated with the political events then impending. We do not recollect since 1830 to have seen

any such exhibition of the Aurora as that visible at a quarter-past eight o'clock on Tuesday night. The moon was up, and the sky was clouded all around the horizon, so was not peculiarly favourable for the exhibition. Towards the zenith, large beams of greenish light shot up: to the south of this an immense tract of sky showed bright carmine. The stars were brightly visible, but tinted with red or green according to the medium through which they were seen. Before nine the aurora had in a great measure disappeared. While it lasted the streets were crowded with spectators to witness the beautiful but almost alarming meteor.—*Perth Constitutional.*

From the Constitution, or Cork Advertizer of Oct, 18*th.*

TO THE EDITOR OF "SAUNDERS'S NEW LETTER."

Sir, Black Rock.

Having seen in your paper of this day a description of the appearance of the Aurora Borealis, on the evening of the 11th, as seen from the north side of our city, and a request that some of your readers on the South side would be so kind as to notice it; I beg leave, therefore, to trespass on your columns to state its appearance here. I noticed it quite accidentally, and at about a quarter-past 8 *p. m.* at which time, however, it seemed to me to be at the north-west, but it appeared as one broad sheet of white light, with now and then a stream shooting up towards the zenith, and the upper part was partly covered by a cirrho-stratus cloud, which, however, I am perfectly confident had no connexion whatever with the aurora, as it was evidently at a great distance above the cloud, and the cloud was carried by the wind towards Howth, where it vanished by degrees. The aurora reached no higher than 50 degrees north, and extended from about 120 degrees to about 210 degrees, being about 90 or 100 degrees from its eastern termination to its western, which latter was dense and well defined. The whole appearance was extremely splendid, and illuminated every object very sensibly. It gradually disappeared, and at a few minutes after nine it was all over.

But of this I am quite certain, the cloud had nothing whatever to do with the aurora, as it was very low in the atmosphere. The wind was westerly at the time, and the sky remarkably clear. If you think this worthy a place in your valuable paper, it will much oblige yours,

October 13th, 1836. W.

Most of the journals of the Departments which have been received to day, in Paris, speak of the *Aurora Borealis* referred to in our number of yesterday. It is astonishing that this phenomenon which was seen simultaneously, at Mentes, Strasburg, Trayes, Rennes, &c., was not observed in Paris.—*Courrier Francois.*

A splendid *Aurora Borealis* appeared last evening about half-past eight o'clock in Rennes, and spread for a moment some alarm in the City. The brightness from the clouds was such that it resembled a large fire breaking through the roof of an edifice. Cries of fire caused the guard to turn out, and the soldiers ran in the direction of the street, Aux Fonlans, and the square St. Anne, to offer their services to extinguish the fire. The *pompiers* were putting their engine in order, when it was discovered that the cause of the alarm was a magnificent *aurora borealis*, which illuminated the sky for several minutes,—*Auxiliarie Breton.*

Strasberg, Oct. 19.—Last evening, between eight and nine o'Clock, a rather extraordinay phenomenon caused a general uproar in our City. A dazzling light suddenly spread over a large extent of the sky, which soon appeared totally on fire. The alarm it occasioned was very great, when it was discovered to be an *Aurora Borealis.* It was a beautiful sight, two immense columns of light arose to a considerable height in the atmosphere, in opposite directions, like two columns of fire. The light was at first pale, but soon after the whole mass appeared to vibrate, and the light, by degrees, became more intense. The phenomenon lasted near half an hour, so that we had full time to contemplate that surprising spectacle.—*Courrier du Bas Rhin.*

Brussels, Oct. 19.—Yesterday evening, at nine o'clock, we had the pleasure of seeing a phenomenon very unusual for this city: viz., the Aurora Borealis. The sky seemed of a blood-red colour over a large space to the N.W. The people crowded the street, and thought it was a fire. It lasted half an hour.—*Morning Herald.*

THE ANNALS

OF

ELECTRICITY, MAGNETISM, AND CHEMISTRY;

AND

Guardian of Experimental Science.

APRIL, 1837.

XXXIII. *Experimental Researches in Electricity—Ninth Series. By* Michael Faraday, *D. C. L. F. R. S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.*

(Continued from page 162*).*

1061. When the helix i or iii (1053. &c.) was used as the connecting conductor, there was also a good spark on breaking contact, but none (sensibly) on making contact. On trying to obtain the shock from these helices, I could not succeed at first. By joining the similar ends of i and ii so as to make the two helices equivalent to one helix, having wire of double thickness, I could just obtain the sensation. Using the helix of thick wire (1055.) the shock was distinctly obtained. On placing the tongue between two plates of silver connected by wires with the parts which the hands had heretofore touched (1064.), there was a powerful shock on *breaking* contact, but none on *making* contact.

1062. The power of producing these phenomena exists therefore in the simple helix, as in the electro-magnet, although by no means in the same high degree.

1063. On putting a bar of soft iron into the helix, it became an electro-magnet (1057.), and its power was instantly and greatly raised. On putting a bar of copper into the helix, no change was produced, the action being that of the helix alone. The two helices i and ii, made into one helix of twofold length of wire, produced a greater effect than either i or ii alone.

1064. On descending from the helix to the mere long wire, the following effects were obtained. A copper wire, 0·18 of

an inch in diameter, and 132 feet in length, was laid out upon the floor of the laboratory, and used as the connecting conductor (1059.): it gave no sensible spark on making contact, but produced a bright one on breaking contact, yet not so bright as that from the helix (1061.). On endeavouring to obtain the electric shock at the moment contact was broken, I could not succeed so as to make it pass through the hands; but by using two silver plates fastened by small wires to the extremity of the principal wire used, and introducing the tongue between those plates, I succeeded in obtaining powerful shocks upon the parts of the mouth, and could easily convulse a flounder, an eel, or a frog. None of these effects could be obtained directly from the electromotor, i. e. when the tongue, frog, or fish was in a similar, and therefore comparative manner, interposed in the course of the communication between the zinc and copper plates, separated everywhere else by the acid used to excite the combination. The bright spark and the shock, produced only on breaking contact, are therefore effects of the same kind as those produced in a higher degree by the helix, and in a still higher degree by the electro-magnet.

1065. In order to compare an extended wire with a helix, the helix i, containing ninety-six feet, and ninety-six feet of the same sized wire lying on the floor of the laboratory, were used alternately as conductors: the former gave a much brighter spark at the moment of disjunction than the latter. Again, twenty-eight feet of copper wire were made up into a helix, and being used gave a good spark on disjunction with the electromotor; being then suddenly pulled out and again employed, it gave a much smaller spark than before, although nothing but its spiral arrangement had been changed.

1066. As the superiority of a helix over a wire is important to the philosophy of the effect, I took particular pains to ascertain the fact with certainty. A wire of copper sixty-seven feet long was bent in the middle so as to form a double termination which could be communicated with the electromotor; one of the halves of this wire was made into a helix and the other remained in its extended condition. When these were used alternately as the connecting wire, the helix gave by much the strongest spark. It even gave a stronger spark than when it and the extended wire were used conjointly as a double conductor.

1067. When a *short wire* is used, *all* these effects disappear. If it be only two or three inches long, a spark can scarcely be perceived on breaking the junction. If it be ten or twelve inches long and moderately thick, a small spark

may be more easily obtained. As the length is increased, the spark becomes proportionately brighter, until from extreme length the resistance offered by the metal as a conductor begins to interfere with the principal result.

1068. The effect of elongation was well shown thus: 114 feet of copper wire, one eighteenth of an inch in diameter, were extended on the floor and used as a conductor: it remained cold, but gave a bright spark on breaking contact. Being crossed so that the two terminations were in contact near the extremities, it was again used as a conductor, only twelve inches now being included in the circuit; the wire became very hot from the greater quantity of electricity passing through it, and yet the spark on breaking contact was scarcely visible. The experiment was repeated with a wire one ninth of an inch in diameter and thirty-six feet long with the same results.

1069. That the effects, and also the action, in all these forms of the experiment are identical, is evident from the manner in which the former can be gradually raised from that produced by the shortest wire to that of the most powerful electro-magnet: and this capability of examining what will happen by the most powerful apparatus, and then experimenting for the same results, or reasoning from them, with the weaker arrangements, is of great advantage in making out the true principles of the phenomena.

1070. The action is evidently dependent upon the wire which serves as a conductor; for it varies as that wire varies in its length or arrangement. The shortest wire may be considered as exhibiting the full effect of spark or shock which the electromotor can produce by its own direct power; all the additional force which the arrangements described can excite being due to some affection of the current, either permanent or momentary, in the wire itself. That it is a *momentary* effect, produced only at the instant of breaking contact, will be fully proved (1089. 1100.)

1071. No change takes place in the quantity or intensity of the current during the time the latter is *continued*, from the moment after contact is made up to that previous to disunion, except what depends upon the increased obstruction offered to the passage of the electricity by a long wire as compared to a short wire. To ascertain this point with regard to *quantity*, the helix i (1053.) and the galvanometer (1058.) were both made parts of the metallic circuit used to connect the plates of a small electromotor, and the deflection at the galvanometer was observed; then a soft iron core was put

into the helix, and as soon as the momentary effect was over, and the needle had become stationary, it was again observed, and found to stand exactly at the same division as before. Thus the quantity passing through the wire when the current was continued was the same either with or without the soft iron, although the peculiar effects occurring at the moment of disjunction were very different in degree under such variation of circumstances.

1072. That the quality of *intensity* belonging to the constant current did not vary with the circumstances favouring the peculiar results under consideration, so as to yield an explanation of those results, was ascertained in the following manner. The current excited by an electromotor was passed through short wires, and its intensity tried by subjecting different substances to its electrolyzing power (912. 966. &c.); it was then passed through the wires of the powerful electromagnet (1056.), and again examined with respect to its intensity by the same means and found unchanged. Again, the constancy of the *quantity* passed in the above experiment (1071.) adds further proof that the intensity could not have varied; for had it been increased upon the introduction of the soft iron, there is every reason to believe that the quantity passed in a given time would also have increased.

1073. The fact is, that under many variations of the experiments, the permanent current *loses* in force as the effects upon breaking contact become *exalted*. This is abundantly evident in the comparative experiments with long and short wires (1068.); and is still more strikingly shown by the following variation. Solder an inch or two in length of fine platina wire (about one hundredth of an inch in diameter) on to one end of the long communicating wire, and also a similar length of the same platina wire on to one end of the short communication; then in comparing the effects of these two communications, make and break contact between the platina terminations and the mercury of the cup G or E (1079.). When the short wire is used, the platina will be *ignited by the constant current*, because of the quantity of electricity, but the spark on breaking contact will be hardly visible; on using the longer communicating wire, which by obstructing will diminish the current, the platina will remain cold whilst the current passes, but give a bright spark at the moment it ceases: thus the strange result is obtained of a diminished spark and shock from the strong current, and increased effects from the weak one. Hence the spark and shock at the moment of disjunction, although resulting from great inten-

sity and quantity of the current *at that moment*, are no direct indicators or measurers of the intensity or quantity of the constant current previously passing, and by which they are ultimately produced.

1074. It is highly important in using the spark as an indication, by its relative brightness, of these effects, to bear in mind certain circumstances connected with its production and appearance. An ordinary electric spark is understood to be the bright appearance of electricity passing suddenly through an interval of air, or other badly conducting matter. A voltaic spark is sometimes of the same nature, but, generally, is due to the ignition and even combustion of a minute portion of a good conductor; and that is especially the case when the electromotor consists of but one or few pairs of plates. This can be very well observed if either or both of the metallic surfaces intended to touch be solid and pointed. The moment they come in contact the current passes; it heats, ignites, and even burns the touching points, and the appearance is as if the spark passed on making contact, whereas it is only a case of ignition by the current, contact being previously made, and is perfectly analogous to the ignition of a fine platina wire connecting the extremities of a voltaic battery.

1075. When mercury constitutes one or both of the surfaces used, the brightness of the spark is greatly increased. But as this effect is due to the action on, and probable combustion of, the metal, such sparks must only be compared with other sparks also taken from mercurial surfaces, and not with such as may be taken, for instance, between surfaces of platina or gold, for then the appearances are far less bright, though the same quantity of electricity be passed. It is not at all unlikely that the commonly occurring circumstance of combustion may affect even the duration of the light; and that sparks taken between mercury, copper, or other combustible bodies, will continue for a period sensibly longer than those passing between platina or gold.

1076. When the end of a short clean copper wire, attached to one plate of an electromotor, is brought down carefully upon a surface of mercury connected with the other plate, a spark, almost continuous, can be obtained. This I refer to a succession of effects of the following nature: first contact,—then ignition of the touching points,—recession of the mercury from the mechanical results of the heat produced at the place of contact and the electro-magnetic condition of the parts at the moment*,—breaking of the contact and the pro-

* Quarterly Journal of Science, vol. xii. p. 420.

duction of the peculiar intense effect dependent thereon,—renewal of the contact by the returning surface of the undulating mercury,—and then a repetition of the same series of effects, and that with such rapidity as to present the appearance of a continued discharge. If a long wire or an electro-magnet be used as the connecting conductor instead of a short wire, a similar appearance may be produced by tapping the vessel containing the mercury and making it vibrate; but the sparks do not usually follow each other so rapidly as to produce an apparently continuous spark, because of the time required when the long wire or electro-magnet is used both for the full development of the current (1101, 1106.) and for its complete cessation.

1077. Returning to the phenomena in question, the first thought that arises in the mind is, that the electricity circulates with something like *momentum* or *inertia* in the wire, and that thus a long wire produces effects at the instant the current is stopped, which a short wire cannot produce. Such an explanation is, however, at once set aside by the fact, that the same length of wire produces the effects in very different degrees, according as it is simply extended, or made into a helix, or forms the circuit of an electro-magnet (1069.). The experiments to be adduced (1089.) will still more strikingly show that the idea of momentum cannot apply.

1078. The bright spark at the electromotor, and the shock in the arms, appeared evidently to be due to *one* current in the long wire, divided into two parts by the double channel afforded through the body and through the electromotor; for that the spark was evolved at the place of disjunction with the electromotor, not by any direct action of the latter, but by a force immediately exerted in the wire of communication, seemed to be without doubt (1070.). It followed, therefore, that by using a better conductor in place of the human body, the *whole* of this extra current might be made to pass at that place; and thus be separated from that which the electromotor could produce by its immediate action, and its *direction* be examined apart from any interference of the original and originating current. This was found to be true: for on connecting the ends of the principal wire together by a cross wire two or three feet in length, applied just where the hands had felt the shock, the whole of the extra current passed by the new channel, and then no better spark than one producible by a short wire was obtained on disjunction at the electromotor.

1079. The *current* thus separated was examined by galvanometers and decomposing apparatus introduced into the course of this wire. I will always speak of it as the current in the

cross wire or wires, so that no mistake, as to its place or origin may occur. In fig. 37, Plate VI. Z and C represent the zinc and copper plates of the electromotor; G and E the cups of mercury where contact is made or broken (1052); A and B the terminations of D the long wire, the helix, or the electro-magnet, used to complete the circuit; N and P are the cross wires, which can either be brought into contact at x, or else have a galvanometer (1058.) or an electrolyzing apparatus (312. 316.) interposed there.

The production of the *shock* from the current in the cross wire, whether D was a long extended wire, or a helix, or an electro-magnet, has been already described (1064. 1061. 1060.)

1080. The *spark* of the cross-wire current could be produced at x in the following manner: D was made an electro-magnet, the metallic extremities at x were held close together or rubbed lightly against each other, whilst contact was broken at G or E. When the communication was perfect at x, little or no spark appeared at G or E. When the condition of vicinity at x was favourable for the result required, a bright spark would pass there at the moment of disjunction, *none* occurring at G and E; this spark was the luminous passage of the extra current through the cross-wires. When there was no contact or passage of current at x, then the spark appeared at G or E, the extra current forcing its way through the electromotor itself. The same results were obtained by the use of the helix or the extended wire at D in place of the electro-magnet.

1081. On introducing a fine platina wire at x, and employing the electro-magnet at D, no visible effects occurred as long as contact was continued; but on breaking contact at G or E, the fine wire was instantly ignited and fused. A longer or thicker wire could be so adjusted at x as to show ignition, without fusion, every time the contact was broken at G or E.

1082. It is rather difficult to obtain this effect with helices or wires, and for very simple reasons: with the helices i, ii, or iii, there was such retardation of the electric current, from the length of wire used, that a full inch of platina wire one fiftieth of an inch in diameter could be retained ignited at the cross-wires during the *continuance of contact*, by the portion of electricity passing through it. Hence it was impossible to distinguish the particular effects at the moments of making or breaking contact from this constant effect.

1083. On using the thick wire helix (1055.), the same results ensued. Proceeding, however, upon the known fact that electric currents of great quantity but low intensity, though able to ignite thick wires, cannot produce that effect upon thin ones, I used a very fine platina wire at x, reducing

its diameter until a spark appeared at G or E, when contact was broken there. A quarter of an inch of such wire might be introduced at x without being ignited by the *continuance* of contact at G or E; but when contact was broken at either place, this wire became red hot; proving, by this method, the production of the induced current at that moment.

1084. *Chemical decomposition* was next effected by the cross-wire current, an electro-magnet being used at D, and a decomposing apparatus, with solution of iodide of potassium in paper (1079.), employed at x. The conducting power of the connecting system A B D was sufficient to carry all the primary current, and consequently no chemical action took place at x during the *continuance* of contact at G and E; but when contact was broken, there was instantly decomposition at x. The iodine appeared against the wire N, and not against the wire P; thus demonstrating that the current through the cross-wires, when contact was broken, was in the *reverse direction* to that marked by the arrow, or that which the electromotor would have sent through it.

1085. In this experiment a bright spark occurs at the place of disjunction, indicating that only a small part of the extra current passed the apparatus at x, because of the small conducting power of the latter.

1086. I found it difficult to obtain the chemical effects with the simple helices and wires, in consequence of the diminished inductive power of these arrangements, and because of the passage of a strong constant current at x whenever a very active electromotor was used (1082.).

1087. The most instructive set of results was obtained, however, when the *galvanometer* was introduced at x. Using an electro-magnet at D, and continuing contact, a current was then indicated by the deflection, proceeding from P to N, in the direction of the arrow; the cross wire serving to carry one part of the electricity excited by the electromotor, and the arrangement A B D, as indicated by the arrows, the other and far greater part. The magnetic needle was then forced back, by pins applied upon opposite sides of its two extremities, to its natural position when uninfluenced by a current; after which, contact being *broken* at G or E, it was deflected strongly in the opposite direction: thus showing, in accordance with the chemical effects (1084.), that the extra current followed a course in the cross-wires *contrary* to that indicated by the arrow, i. e. the one produced by the direct action of the electromotor*.

* It was ascertained experimentally, that if a strong current was passed through the galvanometer only, and the needle restrained in

1088. With the helix only, these effects could scarcely be observed, in consequence of the smaller inductive force of this arrangement, the opposed action from induction in the galvanometer wire itself, the mechanical condition and tension of the needle from the effect of blocking (1087.) whilst the current due to continuance of contact was passing round it, and other causes. With the extended wire all these circumstances had still greater influence, and therefore allowed less chance of success.

1089. These experiments, establishing as they did, by the quantity, intensity, and even direction, a distinction between the primary or generating current and the extra current, led me to conclude that the latter was identical with the induced current described (6, 26.) in the first series of these Researches: and this opinion I was soon able to bring to proof, and at the same time obtained not the partial (1078.) but entire separation of one current from the other.

1090. The double helix (1053.) was arranged so that it should form the connecting wire between the plates of the electromotor, ii being out of the current, and its ends unconnected. In this condition i acted very well, and gave a good spark at the time and place of disjunction. The opposite ends of ii were then connected together so as to form an endless wire, i remaining unchanged: but now no spark, or one scarcely sensible, could be obtained from the latter at the place of disjunction. Then, again, the ends of ii were held so nearly together that any current running round that helix should be rendered visible as a spark; and in this manner a spark was obtained from ii when the junction of i with the electromotor was broken, in place of appearing at the disjoined extremity of i itself.

1091. By introducing a galvanometer or a decomposing apparatus into the circuit formed by the helix ii, I could easily obtain the deflections and decomposition occasioned by the induced current due to the breaking contact at helix i, or even to that occasioned by making contact of that helix with the electromotor; the results in both cases indicating the contrary directions of the two induced currents thus produced (26.).

1092. All these effects, except those of decomposition, were reproduced by two extended long wires, not having the form of helices, but placed close to each other; and thus it

one direction as above in its natural position, when the current was stopped, no vibration of the needle in the opposite direction took place.

was proved that the *extra current* could be removed from the wire carrying the original current to a neighbouring wire, and was at the same time identified, in direction and every respect, with the currents producible by induction (1089.). The case, therefore, of the bright spark and shock on disjunction may now be stated thus: If a current be established in a wire, and another wire, forming a complete circuit, be placed parallel to the first, at the moment the current in the first is stopped it induces a current in the *same* direction in the second, the first exhibiting then but a feeble spark; but if the second wire be away, disjunction of the first wire induces a current in itself in the same direction, producing a strong spark. The strong spark in the single long wire or helix, at the moment of disjunction, is therefore the equivalent of the current which would be produced in a neighbouring wire if such second current were permitted.

1093. Viewing the phenomena as the results of the induction of electrical currents, many of the principles of action, in the former experiments, become far more evident and precise. Thus the different effects of short wires, long wires, helices, and electro-magnets (1069.) may be comprehended. If the inductive action of a wire a foot long upon a collateral wire also a foot in length, be observed, it will be found very small; but if the same current be sent through a wire fifty feet long, it will induce in a neighbouring wire of fifty feet a far more powerful current at the moment of making or breaking contact, each successive foot of wire adding to the sum of action: and by parity of reasoning, a similar effect should take place when the conducting wire is also that in which the induced current is formed: hence the reason why a long wire gives a brighter spark on breaking contact than a short one (1068.), although it carries much less electricity.

1094. If the long wire be made into a helix, it will then be still more effective in producing sparks and shocks on breaking contact; for by the mutual inductive action of the convolutions each aids its neighbour, and will be aided in turn, and the sum of effect will be very greatly increased.

1095. If an electro-magnet be employed, the effect will be still more highly exalted; because the iron, magnetized by the power of the continuing current, will lose its magnetism at the moment the current ceases to pass, and in so doing will tend to produce an electric current in the wire around it (37.38.), in conformity with that which the cessation of current in the helix itself also tends to produce.

1096. By applying the laws of the induction of electric currents formerly developed (6. &c.), various new conditions

of the experiments could be devised, which by their results should serve as tests of the accuracy of the view just given. Thus, if a long wire be doubled, so that the current in the two halves shall have opposite actions, it ought not to give a sensible spark at the moment of disjunction: and this proved to be the case, for a wire forty feet long, covered with silk, being doubled and tied closely together to within four inches of the extremities, when used in that state, gave scarcely a perceptible spark; but being opened out and the parts separated, it gave a very good one. The two helices i and ii being joined at their similar ends, and then used at their other extremities to connect the plates of the electromotor, thus constituted one long helix, of which one half was opposed in direction to the other half: under these circumstances it gave scarcely a sensible spark, even when the soft iron core was within, although containing nearly two hundred feet of wire. When it was made into one consistent helix of the same length of wire it gave a very bright spark.

1097. Similar proofs can be drawn from the mutual inductive action of two separate currents (1110.); and it is important for the general principles that the consistent action of two such currents should be established. Thus, two currents going in the same direction should, if simultaneously stopped, aid each other by their relative influence; or if proceeding in contrary directions, should oppose each other under similar circumstances. I endeavoured at first to obtain two currents from two different electromotors, and passing them through the helices i and ii, tried to effect the disjunctions mechanically at the same moment. But in this I could not succeed; one was always separated before the other, and in that case produced little or no spark, its inductive power being employed in throwing a current round the remaining complete circuit (1090.): the current which was stopped last always gave a bright spark. If it were ever to become needful to ascertain whether two junctions were accurately broken at the same moment, these sparks would afford a test for the purpose, having an infinite degree of perfection.

1098. I was able to prove the points by other expedients. Two short thick wires were selected to serve as terminations, by which contact could be made or broken with the electromotor. The compound helix, consisting of i and ii (1053.), was adjusted so that the extremities of the two helices could be placed in communication with the two terminal wires, in such a manner that the current moving through the thick wires should be divided into two equal portions in the two helices, these portions travelling, according to the mode of connexion, either in the same direction or in contrary direc-

tions at pleasure. In this manner two streams could be obtained, both of which could be stopped simultaneously, because the disjunction could be broken at G or E by removing a single wire. When the helices were in contrary directions, there was scarcely a sensible spark at the place of disjunction; but when they were in accordance there was a very bright one.

1099. The helix i was now used constantly, being sometimes associated, as above, with helix ii in an according direction, and sometimes with helix iii, which was placed at a little distance. The association i and ii. which presented two currents able to affect each other by induction, because of their vicinity, gave a brighter spark than the association i and iii, where the two streams could not exert their mutual influence; but the difference was not so great as I expected.

1100. Thus all the phenomena tend to prove that the effects are due to an inductive action, occurring at the moment when the principal current is stopped. I at one time thought they were due to an action continued during the *continuance* of the current, and expected that a steel magnet would have an influence according to its position in the helix, comparable to that of a soft iron bar, in assisting the effect. This, however, is not the case; for hard steel, or a magnet in the helix, is not so effectual as soft iron; nor does it make any difference how the magnet is placed in the helix, and for very simple reasons, namely, that the effect does not depend upon a permanent state of the core, but a change of state, and that the magnet or hard steel cannot sink through such a difference of state as soft iron, at the moment contact ceases, and therefore cannot produce an equal effect in generating a current of electricity by induction (34. 37.).

1101. As an electric current acts by induction with equal energy at the moment of its commencement as at the moment of its cessation (10. 26.), but in a contrary direction, the reference of the effects under examination to an inductive action, would lead to the conclusion that corresponding effects of an opposite nature must occur in a long wire, a helix, or an electro-magnet, every time that *contact is made* with the electromotor. These effects will tend to establish a resistance for the first moment in the long conductor, producing a result equivalent to the reverse of a shock or a spark. Now it is very difficult to devise means fit for the recognition of such negative results; but as it is probable that some positive effect is produced at the time, if we knew what to expect, I think the few facts bearing upon this subject with which I am acquainted are worth recording.

1102. The electro-magnet was arranged with an electrolyzing apparatus at x, as before described (1084.), except that the intensity of the chemical action at the electromotor was increased until the electric current was just able to produce the feeblest signs of decomposition whilst contact was continued at G and E (1079.); (the iodine of course appearing against the end of the cross wire P;) the wire N was also separated from A at r, so that contact there could be made or broken at pleasure. Under these circumstances the following set of actions was repeated several times; contact was broken at r, then broken at G, next made at r, and lastly renewed at G; thus any current from N to P due to *breaking* of contact was avoided, but any additional force to the current from P to N due to *making* contact could be observed. In this way it was found, that a much greater decomposing effect (causing the evolution of iodine against P) could be obtained by a few completions of contact than by the current which could pass in a much longer time if the contact was *continued.* This I attribute to the act of induction in the wire A B D at the moment of contact rendering that wire a worse conductor, or rather retarding the passage of the electricity through it for the instant, and so throwing a greater quantity of the electricity which the electromotor could produce, through the cross wire passage N P. The instant the induction ceased, A B D resumed its full power of carrying a constant current of electricity and could have it highly increased, as we know by the former experiments (1060.) by the opposite inductive action brought into activity at the moment contact at Z or C was *broken.*

1103. A galvanometer was then introduced at x, and the deflection of the needle noted whilst contact was continued at G and E; the needle was then blocked as before in one direction (1057.), so that it should not return when the current ceased, but remain in the position in which the current could retain it. Contact at G or E was broken, producing of course no visible effect; it was then renewed, and the needle was instantly deflected, passing from the blocking-pins to a position still further from its natural place than that which the constant current could give, and thus showing by the temporary excess of current in this cross communication, the retardation in the circuit A B D.

1104. On adjusting a platina wire at x (1081.) so that it should not be ignited by the current passing through it whilst contact at G and E was *continued,* and yet become red hot by a current somewhat more powerful, I was readily able to produce its ignition upon *making contact,* and again upon

breaking contact. Thus the momentary retardation in A B D on making contact was again shown by this result, as well also as the opposite result upon breaking contact. The two ignitions of the wire at *x* were of course produced by electric currents moving in opposite directions.

1105. Using the *helix* only, I could not obtain distinct deflections at *x*, due to the extra effect on making contact, for the reasons already mentioned (1088.). By using a very fine platina wire there (1083.), I did succeed in obtaining the igniting effect for making contact in the same manner, though by no means to the same degree, as with the electro-magnet (1104.).

1106. We may also consider and estimate the effect on *making contact*, by transferring the force of induction from the wire carrying the original current to a lateral wire, as in the cases described (1090.): and we then are sure, both by the chemical and galvanometrical results (1091.), that the forces upon making and breaking contact, like action and reaction, are equal in their strength but contrary in their direction. If, therefore, the effect on making contact resolves itself into a mere retardation of the current at the first moment of its existence, it must be, in its degree, equivalent to the high exaltation of that same current at the moment contact is broken.

1107. Thus the case, under the circumstances, is, that the intensity and quantity of electricity moving in a current are smaller when the current commences or is increased, and greater when it diminishes or ceases, than they would be if the inductive action occurring at these moments did not take place; or than they are in the original current wire if the inductive action be transferred from that wire to a collateral one (1090.).

1108. From the facility of transference to neighbouring wires, and from the effects generally, the inductive forces appear to be lateral, *i. e.* exerted in a direction perpendicular to the direction of the originating and produced currents; and they also appear to be accurately represented by the magnetic curves, and closely related to, if not identical with, magnetic forces.

1109. There can be no doubt that the current in one part of a wire can act by induction upon other parts of the *same* wire which are lateral to the first, *i. e.* in the same section, or in the parts which are more or less oblique to it (1112.), just as it can act in producing a current in a neighbouring wire. It is this which gives the appearance of the current acting upon itself: but all the experiments and all analogy tend to

show that the elements (if I may say so) of the currents do not act upon themselves, and so cause the effect in question, but produces it by exciting currents in conducting matter which is lateral to them.

1110. It is possible that some of the expressions I have used may seem to imply, that the inductive action is essentially the action of one current upon another, or of one element of a current upon another element of the same current. To avoid any such conclusion I must explain more distinctly my meaning. If an endless wire be taken, we have the means of generating a current in it which shall run round the circuit without adding any electricity to what was previously in the wire. As far as we can judge, the electricity which appears as a current is the same as that which before was quiescent in the wire; and though we cannot as yet point out the essential condition of difference of the electricity at such times, we can easily recognise the two states. Now when a current acts by induction upon conducting matter lateral to it, it probably acts upon the electricity in that conducting matter whether it be in the form of a *current* or *quiescent*, in the one case increasing or diminishing the current according to its direction, in the other producing a current, and the *amount* of the inductive action is probably the same in both cases. Hence, to say that the action of induction depended upon the mutual relation of two or more currents, would, according to the restricted sense in which the term current is understood at present (283. 517. 667.), be an error.

1111. Several of the effects, as, for instances, those with helices (1066.), with according or counter currents (1097. 1098.), and those on the production of lateral currents (1090), appeared to indicate that a current could produce an effect of induction in a neighbouring wire more readily than in its own carrying wire, in which case it might be expected that some variation of result would be produced if a bundle of wires were used as a conductor instead of a single wire. In consequence the following experiments were made. A copper wire one twenty-third of an inch in diameter was cut into lengths of five feet each, and six of these being laid side by side in one bundle, had their opposite extremities soldered to two terminal pieces of copper. This arrangement could be used as a discharging wire, but the general current could be divided into six parallel streams, which might be brought close together, or, by the separation of the wires, be taken more or less out of each other's influence. A somewhat brighter spark was, I think, obtained on breaking contact when the six wires were close together than when held asunder.

1112. Another bundle, containing twenty of these wires, was eighteen feet long: the terminal pieces were one fifth of an inch in diameter, and each six inches long. This was compared with nineteen feet in length of copper wire one fifth of an inch in diameter. The bundle gave a smaller spark on breaking contact than the latter, even when its strands were held together by string; when they were separated, it gave a still smaller spark. Upon the whole, however, the diminution of effect was not such as I expected; and I doubt whether the results can be considered as any proof of the truth of the supposition which gave rise to them.

1113. The inductive force by which two elements of one current (1109. 1110.) act upon each other, appears to diminish as the line joining them becomes oblique to the direction of the current, and to vanish entirely when it is parallel. I am led by some results to suspect that it then even passes into the repulsive force noticed by AMPERE*; which is the cause of the elevations in mercury described by SIR HUMPHRY DAVY†, and which again is probably directly connected with the quality of intensity.

1114. Notwithstanding that the effects appear only at the making and breaking of contact, (the current remaining unaffected, seemingly, in the interval,) I cannot resist the impression that there is some connected and correspondent effect produced by this lateral action of the elements of the electric stream during the time of its continuance (60. 242.). An action of this kind, in fact, is evident in the magnetic relations of the parts of the current. But admitting (as we may do for the moment) the magnetic forces to constitute the power which produces such striking and different results at the commencement and termination of a current, still there appears to be a link in the chain of effects, a wheel in the physical mechanism of the action, as yet unrecognised. If we endeavour to consider electricity and magnetism as the results of two forces of a physical agent, or a peculiar condition of matter, exerted in determinate directions perpendicular to each other, then, it appears to me, that we must consider these two states or forces as convertible into each other in a greater or smaller degree; i. e. that an element of an electric current has not a determinate electric force and a determinate magnetic force constantly existing in the same ratio, but that the two forces are, to a certain degree, convertible by a process or change of condition at present unknown to us. How

* Recueil d'Observations Electro-Dynamiques, p. 285.

† Philosophical Transactions, 1823, p. 155.

else can a current of a given intensity and quantity be able, by its direct action, to sustain a state which, when allowed to react, (at the cessation of the original current,) shall produce a second current, having an intensity and quantity far greater than the generating one? This cannot result from a direct reaction of the electric force; and if it result from a change of electrical into magnetic force, and a reconversion back again, it will show that they differ in something more than mere direction, as regards *that agent* in the conducting wire which constitutes their immediate cause.

1115. With reference to the appearance, at different times, of the contrary effects produced by the making and breaking contact, and their separation by an intermediate and indifferent state, this separation is probably more apparent than real. If the conduction of electricity be effected by vibrations, or by any other mode in which opposite forces are successively and rapidly excited and neutralized, then we might expect a peculiar and contrary development of force at the commencement and termination of the periods during which the conducting action should last (somewhat in analogy with the colours produced at the outside of an imperfectly developed solar spectrum): and the intermediate actions, although not sensible in the same way, may constitute the very essence of conductibility. It is by views and reasons such as these, which seem to me connected with the fundamental laws and facts of electrical science, that I have been induced to enter, more minutely than I otherwise should have done, into the experimental examination of the phenomena described in this paper.

1116. Before concluding, I may briefly remark, that on using a voltaic battery of fifty pairs of plates instead of a single pair (1052.), the effects were exactly of the same kind. The spark on making contact, for the reasons before given, was very small (1101. 1107.); that on breaking contact, very excellent and brilliant. The *continuous* discharge did not seem altered in character, whether a short wire or the powerful electro-magnet were used as a connecting discharger.

1117. The effects produced at the commencement and end of a current, (which are separated by an interval of time when that current is supplied from a voltaic apparatus,) must occur at the same moment when a common electric discharge is passed through a long wire. Whether, if happening accurately at the same moment, they would entirely neutralize each other, or whether they would not still give some definite peculiarity to the discharge, is a matter remaining to be examined; but it is very probable that the peculiar character

and pungency of sparks drawn from a long wire depend in part upon the increased intensity given at the termination of the discharge by the inductive action then occurring.

1118. In the wire of the helix of magneto-electric machines, (as, for instance, in Mr. SAXTON's beautiful arrangement,) an important influence of these principles of action is evidently shown. From the construction of the apparatus the current is permitted to move in a complete metallic circuit of great length during the first instants of its formation: it gradually rises in strength, and is then suddenly stopped by the breaking of the metallic circuit; and thus great intensity is given *by induction* to the electricity, which at that moment passes (1064. 1060.). This intensity is not only shown by the brilliancy of the spark and the strength of the shock, but also by the necessity which has been experienced of well insulating the convolutions of the helix, in which the current is formed; and it gives to the current a force at these moments very far above that which the apparatus could produce if the principle which forms the subject of this paper were not called into play.

Royal Institution,
December 8th, 1834.

Remarks on the preceding paper, with Experiments.

Dr. Faraday does not appear to have arrived at any distinct views respecting the nature of the action in the display of these curious phenomena (see paragraphs 1114, and 1115.). "Magnetic forces" are spoken of in paragraph 1108, but not in that language which conveys any definite meaning. Nor are we better supplied with information by the frequency of the term "induction", which, in the manner there used implies nothing farther than that *some* force is in operation; but as to what that force is, or in what manner it performs its operations, are matters not attempted to be explained, and indeed are such, as the word "induction" alone, is not calculated to express.

With regard to Dr. Faraday's experiments on the effects of soft iron in the process, they were obviously too limited, and not varied with that caution, and to that extent, to be productive of any other than unconnected results; which, though highly interesting as insulated facts, were not calculated to establish any general law. From the shock and spark appearing to be exalted by the introduction of the electro-magnet (1056, 1060) to the circuit, Dr. Faraday has been led to think that the display of these phenomena are proportional

to the development of the ferreous magnetic energies, an error obviously arising from a want of due attention to comparative experiments, as may be seen by a perusal of the next article.

The terminal current producing decomposition (1084) proceeding in the *reverse direction* to that marked by the arrow N P, fig. 37, is no indication of the original battery current in the helix at D, being inverted; but on the contrary, it is a proof of its being continued in the same direction B, D, A, *r*, *x*, *&c.*, and running into itself, because of the battery being cut off by the opening made at E, or at G, and in no instance does the direction of the two differ.

The inferences which Dr. Faraday has drawn from his experiment detailed in paragraph 1103, are the most unfortunate that could possibly have been hit upon, resting principally, if not solely, upon the idea of *permanent* and *transient* deflections being of the same extent, from the same source of electro-dynamic action, the incorrectness of which has been amply proved in the first number of these "Annals." See our *enquiry into the attributes of the Galvanometer.* Let the magnetic needle of any galvanometer be steadily deflected by any voltaic current: and then, by any means most convenient, let it be prevented from returning to the meridian when the electric current is cut off; observing that its freedom to motion in the opposite direction is not obstructed. This done, let the battery connexion be suddenly made, and the needle will be instantly deflected over a considerable arc, beyond its last stationary position. This will always happen whether there be one, two, or more metallic circuits for the electric fluid to flow through; but the deflection will always be greatest when there is only one channel.

Let us, for instance, vary Dr. Faraday's experiment, by simply removing the long helical wire, or electro-magnet which he employed, from D, leaving that circuit only which passes through the galvanometer at *x*, fig. 37. Let the needle now take a stationary position by a current E, P, *x*, N, G, &c. from *c* to *z*. The needle being retained by some means, in this position, let the circuit be opened at E or G. Now again close the circuit, and the needle will be deflected over a greater arc than if the helix, or electro-magnet, had been in connexion at D. These results lead to conclusions the very reverse of those which Dr. Faraday has drawn from the *partial*, and consequently *inconclusive* character of his experimental data: but they are precisely such as might have been expected by any one conversant with the nature of electro-dynamic action. A cessation of this action, even a

momentary pause, invariably improves the powers of a battery; hence one cause of the needle's deflection from its stationary position.—The recruited force of the battery, whatever may be its extent, will exert more electro-dynamic action in any one circuit when alone, than when accompanied by other circuits ready to transmit a portion of the force; hence the greater deflection *without* the long circuit at D, than with it.

In all experiments of this kind the powers of the battery should be kept as uniform as possible. Uniformity of action, however, is not easily maintained by active batteries of copper and zinc, but is easily attained when the action is low, either by employing salt water, or feeble dilute acid, and especially if the plates be not very clean. When amalgamated zinc is employed, with copper, and the liquid dilute sulphuric acid, an uniformity of action may be maintained for a long time.

When an active battery is employed, the *stationary* deflection of the needle will be some degrees greater *without* the coil circuit at D, than with it: but if the battery be so feeble that the stationary angle without the coil does not exceed 15° the introduction of the coil circuit, if above 100 feet long, does not reduce the angle more than one or two degrees.

The transient powers of batteries in feeble action depend very much upon the interval of their repose, improving every moment for the first minute at least, perhaps for a longer period; hence, in making comparative experiments by transient deflections, some standard interval of time for the battery's repose between every two experiments, ought to be strictly attended to.

The following table of *transient* deflections are the results of a series of experiments with a battery of a single pair of copper and zinc, the electro-dynamic action of which maintained a steady deflection of 12°, not varying 1° either with or without an additional coil circuit of 110 feet of copper wire, as at D, fig. 37. The galvanometer used was that described at page 48 of this volume. The interval of the battery's repose, prior to each experiment, was one minute; and the needle started each time from the meridian line of the graduated card. The experiments were made by pairs, one of each of which *with* the coil circuit, the other *without* the coil. The battery was left in connexion each time until the needle had reached its highest point of deflection.

coil	64°	coil	56°	coil	50°
no coil	68	no coil	60	no coil	55
coil	63	coil	55		
no coil	65	no coil	63		
coil	50	coil	55		
no coil	64	no coil	62		
coil	55	coil	54		
no coil	63	no coil	59		

These experiments amply prove the superiority of transient electro-dynamic action in a single circuit, when alone, over that in the same circuit when other channels of transmission are open. They prove also, if any farther proof were necessary, all that has been advanced* respecting the distinctive characters of *transient* and *permanent* deflections from the same source of electric action. The interval of action in each of these experiments was about 3′ of time. In no case is the arc of *transient* deflection less than four times that of *permanent* deflection; and in some of the experiments the arc of the former is more than five times that of the latter.

That there is, however, a virtual resistance in a coil conductor to the first efforts of an electric current, although not proved by Dr. Faraday's experiments, has appeared to me from the first so highly probable that I have left no means untried, which have occurred to me as being likely to set this important theoretical point at rest.

In the postscript to my first paper on this subject, which was hastily written one evening after the fatigue of a whole day's experimenting, I had barely time to allude to this supposed resistance,† without meditating on any experiment to ascertain the correctness or incorrectness of my views concerning it. When I first read Dr. Faraday's paper, towards the latter end of October last, I was much interested in finding that similar views had been taken by the author; but notwithstanding the flattering conclusions there arrived at, I must confess that, at first glance, the mode of investigation appeared to be so very imperfect that any inference drawn from it must obviously be too gratuitous to be relied on.

The decompositions and heating effects in Dr. Faraday's experiments (1102, 1104), are so far from being satisfactory

* See pages 48 and 49 of this volume.
† See page 74 of this volume.

results that the whole of them might be referred to the same cause as the deflections of the needle (1103). That is, to an improved power in the battery during the temporary interruptions in the circuit; and the assertion in 1105 is entirely without foundation, as may easily be understood by the experiments I have already described.

Whilst thinking of various experiments for this curious enquiry, it eventually occurred to me that as the temporary resistance in a long wire formed into a compact coil ought to be greater than that in a similar wire, either quite straight or loosely hung in any convenient manner that would prevent its folds from exercising an action on each other, this superior resistance in the coil, whatever might be its amount, would tend to lessen the *transient* deflecting force, and its existence might probably be indicated by lessening the deflections of the needle. Under these impressions the following arrangements were made for experiments.

The galvanometer used was that already mentioned, and the battery similar to that employed in the last described experiments with the zinc amalgamated, the liquid being very dilute sulphuric acid. The two conducting wires were each 300 feet long, of the same dimensions, and well covered with silk. One of these wires was formed into a compact coil, similar to one of those represented in fig. 16, Plate 2. The reel wood, its cylindric nucleus about two inches diameter, and the same in length. The other wire was hung in loose folds on chairs.

These wires were used separately, each forming 300 feet of the conducting circuit for two experiments; then removed and the other introduced for two experiments, and so on. The steady deflection when either the coil or the uncoiled wire was in the circuit was about 20°, not varying half a degree under the two circumstances. Several trials to ascertain the stationary deflections were made during the time the experiments were carrying on, from which the battery was found to maintain a steady power. The plates of the battery were never disturbed the whole time. The interval of repose was one minute prior to each experiment. The interval of electric action in each experiment was 2½″. The needle during this time had always attained its highest point of deflection. The needle started each time from the meridian line of the card.

Tables of transient deflections with the coiled and with the uncoiled wire in the circuit.

300 feet coiled.		300 feet uncoiled.	
Exp.	Deflection.	Exp.	Deflection.
1 - - - -	59°	3 - - -	65°
2 - - - -	60°	4 - - -	70°
5 - - - -	65°	7 - - -	69°
6 - - - -	65°	8 - - -	70°
9 - - - -	65°	11 - - -	69°
10 - - - -	65°	12 - - -	70°
13 - - - -	60°	15 - - -	66°
14 - - - -	62°	16 - - -	67°
17 - - - -	60	19 - - -	67°
18 - - - -	60	20 - - -	68°
mean - - -	62·5°	mean - - -	68·5°

By taking the forces as the sines of half the arcs of mean deflection, we have those forces as 1·08487 : 1, nearly. And if the resistances in the wires be reciprocally as the deflecting forces, then the resistance in the coil to that in the loose wire will be as 1·08487 : 1.

With other wires and other electric powers the ratio of resistance would, doubtless, be found to be very different to that exhibited by these experiments. But although this is the only series of experiments I have yet made to determine this point, the care with which they were conducted leads me to believe, that in all cases the resistance will be found greater in a coiled wire, than in an uncoiled one of the same dimensions.

W. S.

XXXIV. *An Experimental Investigation of the Laws which govern the production of Electric Shocks, &c. from a single Voltaic pair of metals.* By WILLIAM STURGEON, *Lecturer on Experimental Philosophy, at the Honourable East India Company's Military Seminary, Addiscombe.*

From the disagreement of results in some of the experiments detailed by Dr. Faraday, and those described in my paper published in the first number of these Annals,* I have been induced to look again at what I had done, and to repeat those experiments in which the difference in our results was most conspicuous.

In comparing our experiments, however, I have been enabled to perceive that in fact they were not exactly of the same character, but differ essentially from each other by reason of the difference of the apparatus we employed. I have already noticed in the preface to Dr. Faraday's paper,† that I then suspected that the disagreement of the results of our experiments, when iron was employed, was probably owing to the fashion of our coils, which I now find has a considerable influence over the results. I have also been led to the discovery of other circumstances which, by modifying the phenomena, become of considerable interest in the theory of the action.

The iron which I first employed was the rotating armature of a magnetic electrical machine, and the coil of 300 feet of copper wire was placed on one of its branches, and when carrying an electric current the armature was converted into a temporary horse-shoe magnet. By this application of the iron, I could perceive no increase in the power of the shocks, they being of equal intensity when the iron was not present. This experiment I have repeated and the results are similar to those I first noticed.

I next instituted a new series of experiments with coils and iron of other fashions.

Experiment 1. Sixty feet of copper wire one twentieth of an inch diameter, and well covered with silk were made into a helix on a card-board tube which would just admit a cylindric iron bar of half an inch diameter. The helix was nine inches long, and consisted of three strata of close packed coils from end to end. The iron bar was twelve inches long. The

* Commencing at page 67.
† No. 2, page 160.

metals forming the battery were cylinders of copper and zinc, the former four inches diameter, the latter about three, and each eight inches high; they were placed in a porcelain jar so that the exciting liquid could have access to all the surfaces. The liquid salt and water cold. With this battery and the helix in the circuit I could perceive no shocks either *with* or *without* the iron. The sparks when shown at the surface of mercury were alike in both cases, so were the scintillations by running a fine iron wire termination over the surface of a properly connected plate similar to that shown by fig. 36.

Experiment 2. The helix being removed, the wires of an electro-magnet were next placed in the circuit. These wires were twelve in number, and the average length of each about forty feet. They formed twelve distinct coils, one above another, round an iron bar, bent into the form of a horse-shoe magnet, whose branches have scarcely any curvature from the central bend to their extremities, which are about an inch and a-half asunder. The bar weighs sixteen pounds. No shocks could be obtained by this arrangement although the magnetic power at the poles amounted to about thirty pounds. The spark was small.

Experiment 3. The electro-magnet being removed, a coil of 300 feet of copper wire was introduced. The wire of this coil was about the same diameter as that of the coil in experiment 1, and well covered with silk. The coil was formed on a wooden bobbin whose inner diameter was about two inches, and the length the same. The shocks were pretty strong; iron wire scintillated; the spark bright.

Experiment 4. The same battery metals were again employed. The liquid salt and water nearly boiling hot. When the first mentioned coil of sixty feet of wire (ex. 1.) was placed in the circuit, no shocks could be obtained. When the iron cylinder was placed in the coil, slight tinglings were felt in the fingers of the same hand when dipped into salt water in two small jars properly connected.* The spark not perceptibly different with, or without the iron.

Experiment 5. When the wires of the large electro-mag-

* When the power is very feeble, it is useful to know that the sensation is best obtained in the little finger, when immersed *alone* in one of the portions of salt water, and the other fingers and thumb in the other portion. With more powerful apparatus, the shocks are insufferably painful, when one hand is immersed in one portion of salt and water, and the little finger of the other hand immersed in the other portion; both being properly connected.

net (ex. 2.) formed the circuit of this hot battery, no shock could be obtained; but a bright spark was observed whenever contact was broken. The magnet in this case would lift seventy pounds.

Experiment 6. When the short thick coil of 300 feet of wire (ex. 3.) formed the circuit, the shocks were more powerful than with the cold solution; and the sparks and scintillations much brighter.

Experiment 7. A smaller battery was now used, rendered very active by nitrous acid and water. With the coil of sixty feet of wire (ex. 1.) and no iron, no shock could be felt. When the iron bar was introduced, smart tinglings in the fingers. The spark was nearly alike in both cases.

Experiment 8. With the large electro-magnetic wires (ex. 2.) in the circuit, no shock could be felt. The sparks and scintillations much brighter than with the other battery. The magnet in this case would lift more than two hundred pounds.

Experiment 9. When the electro-magnet was removed, and the coil of 300 feet placed in the circuit, smart shocks and bright sparks were obtained.

Experiment 10. Another helix was now made of 110 feet of copper wire similar to that in the former helices. This helix was nine inches long, formed on a pasteboard tube which would just admit a cylindric iron bar of one inch diameter. The iron cylinder was twelve inches long. A new zinc was made for the large battery, (ex. 1.) and the liquid employed was cold salt and water. This helix without the iron gave no shock; with the iron, gentle tinglings only. The sparks were nearly alike in both cases.

Experiment 11. The battery in this experiment was brought to great activity by nitrous acid and water; the same helix (ex. 10.) again forming the circuit. Without the iron no shocks could be obtained. With the iron in the helix slight shocks were perceptible. The sparks and scintillations nearly alike in both cases.

Experiment 12. When the wires of the electro-magnet (ex. 2.) formed the circuit of this active battery, no shocks could be obtained. The sparks and scintillations were exceedingly fine. The magnet in this case would lift four hundred pounds.

Experiment 13. When the coil of 300 feet of wire (ex. 3.) formed the circuit, the shocks were very smart.

Remarks. In all the hitherto described experiments the coil of 300 feet of wire gave much the strongest shocks,

although no iron was connected with it; proving in the most ample manner that the magnetism of the iron employed is not the *sole* cause of the shocks.

That shocks should be produced by 60 feet of wire and none by 480 feet, although the latter was aided by a ferreous magnetic power more than 300 times that of the former (compare ex. 4 and 12.) is a fact exceedingly curious, and one which could hardly have been predicted by those who have referred the principal operating power to the magnetic action of the enclosed iron; and that the coil without any iron whatever, should produce stronger shocks than the coils with iron, is a fact still more at variance with those views.

It is obvious, however, by these experiments, that the magnetism of the iron under some circumstances becomes efficient, and therefore the principal mystery rests in its not being efficient in all cases; and how it should fail when produced in greatest abundance. Whilst contemplating these facts, it occurred to me that the cause of the superiority of the 300 feet coil over the other arrangements, might probably be traced partly to the greater length of circuit, and partly to the fashion of the coil; and if so, similar coils with the same length of wire, the one *with* and the other *without* an iron nucleus, ought to show a difference of action. This, however, had already been done, under some circumstances, in my previous experiments, without being productive of much information on this point. There still, however, seemed a probability of the figure of the iron being concerned in the process, and especially if the action was that of magnetic-electricity, as the results of this last series of experiments had partly indicated.

In magnetic-electricity it is well known that the shocks principally depend upon the length of the coil wires; at least up to a certain extent. But the sparks and calorific effects are best developed by shorter and stouter wires; or which amounts to the same thing, by shorter wires, and more of them. This is in exact accordance with the facts developed by the experiments I have last detailed. The strongest shocks were obtained from the longest circuit, but the largest sparks, and brightest scintillations, from the greatest quantity of conducting matter in the circuit, in comparatively short lengths.

With these novel views I entertained hopes of being enabled to modify the effects;—from the iron by giving to it different forms, and from the wire by altering the fashion of the coils only, without any variation in the figure of the iron. Proceeding to experimental investigation, the first point I wished to determine, was that of the influence of a straight bar in a long con-

ducting wire. This could not be very satisfactorily ascertained only by the employment of a battery of steady and uniform action during the period occupied by the experiments, which were carried on in the following manner.

Experiment 14. A new zinc was now made for the smaller battery. The zinc amalgamated, and the exciting liquid dilute sulphuric acid. A wire 300 feet long, and similar in every respect to that coiled on the wooden bobbin (ex. 3.), was wound round about two inches and a half of the central part of a cylindric bar of iron, one inch in diameter, and twelve inches long. This coil and that on the wooden bobbin were alternately, for twenty times, placed in the circuit of the battery. In every trial the coil round the iron gave the greater shock.

Experiment 15. The question now to be decided, was, can the action of the iron be made null when covered with one of these wires? Or can it be made to operate in a negative capacity, by lessening the force which the coil alone would exhibit?

To ascertain this point a similar bar of iron to that used in the last experiment was bent in the middle and formed like a horse-shoe magnet, with its two branches as straight and parallel to each other as they could conveniently be made. They were also brought pretty close to each other; all these particulars being considered to be essentially concerned in the action. This piece of iron was covered with the 300 feet of wire which had previously formed the coil on the wooden bobbin (ex. 3.). The wire formed six strata of coils whose convolutions were close packed together. When this coil wire formed the circuit of the last mentioned battery (ex. 14.) the electro-magnet would carry upwards of eighty pounds. It was now placed in the circuit, alternately with that wound round the central part of the straight bar, (ex. 14.) and shocks taken more than twenty times from each. In every trial the shocks were much the strongest from the coil on the straight bar; showing again that the shocks do not depend upon the *quantity* of magnetism displayed, but upon a proper application of it.

Experiment 16. The wire was taken off the straight iron bar and wound in a close packed coil on the wooden bobbin. This done, the coil now formed was tried against that round the horse-shoe (ex. 15.) and found to be of superior efficacy in producing shocks.

Nothing could be more decisive than the results of these experiments, in proving that that form of the iron most suita-

ble for magnetic display, is the least so, in the production of shocks. They prove also, that the iron may even be detrimental when used for the latter purpose.

Notwithstanding the satisfaction which I felt in my own mind respecting the lessened influence of the iron by bending it, there yet appeared one circumstance connected with the experiments, which, because of the possibility of its being a means of modifying the results, might probably create doubts respecting the conclusions I have arrived at. It will have been observed that in making the experiments with the straight and bent iron bars (ex. 15.), the two coils of conducting wire which were formed on them were of very different fashions. That on the straight bar being a short thick coil, whilst that on the horse-shoe bar was much longer and thinner, covering the iron from one end to the other.

Experiment 17. To prevent any misunderstanding arising from the above circumstance, I took the 300 feet of wire off the wooden bobbin, and coiled it round the straight iron cylinder, in, as nearly as possible, the same manner as that coiled round the bent one, it being impracticable to make both helices precisely alike, because of the different shapes of the iron. This coil and that on the bent bar were alternately placed in the circuit of a battery of steady action, for several successive times, and shocks taken from them individually. Those from the coil enclosing the straight bar, were, in every trial, much stronger than those from the other coil. In this case, the coils were of the same fashion, the wires forming them of the same length, and the pieces of iron of the same dimensions.

Experiment 18. This experiment was intended to determine the only remaining point which appeared to be of much interest respecting the *modus operandi* in these curious phenomena, and the results show most decidedly that the fashion and position of the coil on the same piece of iron have considerable influence in modifying the phenomena.

The 300 feet of wire were taken off the bent iron bar (ex. 15.) and wound in a close packed coil round the central part of a straight iron cylinder of precisely the same dimensions as the former (ex. 14.) and the coil of the same fashion as there described.

This coil, and that covering the other straight iron cylinder (ex. 17.), were alternately placed in the circuit of a battery of steady action for twenty successive times, and shocks taken from them individually every time. In every trial the shock from the short thick coil was much stronger than that from the long coil which covered nearly the whole length of its en-

closed iron. This is a very interesting fact, as will appear obvious from a due consideration of some of the circumstances connected with it.

For instance, the iron enclosed in the long helix became a more powerful magnet than that round whose centre the short thick helix was formed. Moreover, the mean distance, of the wire forming the *long* helix, from the iron, was much less than the mean distance of the wire forming the short helix from its iron; which, as far as the magnetism of the iron is concerned, is another advantage.

From my first experiencing the shock from a coil, the effect appeared to me to be that of an electro-momentum; but from the hurried manner in which my experiments were made and described, I had not sufficient time allowed to consider the nature of the action with that care which it evidently demanded. The more recent investigations which I have now detailed, having furnished more varied theoretical data I have been better enabled to perceive the connexion of the phenomena and their causes; and to enhance the display by a proper application of the laws which govern the action. Still viewing the shocks as the effects of electro-momenta, I was again led to try double conducting wires, and have found, that, under certain circumstances, the shock is more powerful than with one wire only. Three or four wires may be advantageously employed, provided certain rules be attended to.

The length of the circuit most efficient in the production of shocks will depend upon the intensity of the battery: and the number of strands to be introduced to the circuit must be regulated to the extent of the battery's surface.

XXXV. *Theoretical Views of the preceding Phenomena. Secondary Electric Currents, &c.*

The singular influence which an electric current exercises in bringing into momentary activity the dormant electric energies of an adjacent wire, requires considerable attentiveness and much thought to comprehend the manner of its action. Its contemplation requires a previous full knowledge of the proximate laws which govern the reciprocal excitation of electrics and magnetics; and the most profound ideas respecting the operation of these laws in the invisible processes by which those powers are productive of each other's phenomena.

Induction,—influence,—reaction, &c, although satisfactory enough to express, generally, that *some* force is in operation, give no intelligence whatever respecting the nature of that

force, and, consequently, indicate no mode of its action. To *guess* that this or that power is in operation may possibly stimulate to enquiry, and occasionally become useful in that humble capacity. But conjecture without principles implies imperfect knowledge, and must never be regarded as the offspring of sound philosophical reasoning.

Facts may be produced and phenomena predicted by those who are in possession of certain rules which have become established from observation; but the primary laws from which these rules and those phenomena emanate are existences of a very different order. The perfect invisibility of the process by which these laws operate precludes its recognition by the external senses: and renders it comprehensible to the mind only. All our reasoning, however, on the invisible operations of nature must necessarily be based on those which, by their conspicuousness, have become perfectly familiar to us: so that by applying the one to the phenomena of the other we may be enabled to ascertain whether or no the same laws be applicable in both cases.

That the laws which govern the production of secondary electric currents by the influence of primary ones are still obscured in mystery, may be justly inferred from the fact that no attempt has hitherto appeared to throw the least gleam of light on their development. The principles of action may possibly have appeared too recondite for development, but can never be considered as too uninteresting to deserve attention. The whole theory of electro-magnetism and magnetic-electricity hangs upon them.

Why, it may now be asked, do *secondary* currents run counter to *primitive* currents by the first impulses of the latter; and in the same direction as the primitive by the last impulses? Why, also, are the energies of the first-named secondaries much feebler than those of the latter secondaries? And why do secondaries almost annihilate the *terminal* effects of primitives? These facts, which are developed either with or without iron, have not hitherto been referred to any definite cause; nor, indeed, has any attempt yet been made at explanation. They are, it is true, amongst the most mysterious phenomena presented by this branch of physics, and the laws by which they are exhibited the most difficult of access; and interwoven with curious and intricate complexity; but there are others of less difficult explanation, whose sources of action are still permitted to remain in concealment. The equality of *action* and *reaction*, a law so generally admitted into reasonings on physics, although not refuted by these curious phenomena, afford no assistance in the solution of the mys-

terious problems which they present. The renitency encountered in the conductors will necessarily exercise a due influence in lessening the force of secondary currents, but cannot be made available as a cause of the comparative atony which these currents, by the *initial* impulses of the primary, invariably display.

The laws which govern these interesting phenomena do not, however, appear to be too deeply hidden for recognition: and although, in some instances, a complexity of action is discoverable, which tends to conceal the operating forces, they do not appear to me to be entirely precluded from access, nor insusceptible of explication. Electricity and magnetism are here, however, playing their nimble powers on each other in the most profound retirement:—their motions concealed from corporeal vision permit of no other approach than by the perceptions of the mind: and by that mind only, already perfectly familiar with the proximate laws of magnetic-electricity.

From this circumstance the propriety of introducing these laws prior to entering on the development of the more remote subtle operations of the electric and magnetic powers will appear sufficiently obvious.

(To be continued.)

XXXVI.—OXY-HYDROGEN BLOWPIPE.

The Silver Isis Medal was presented to Mr. W. Maugham, *for his oxy-hydrogen blowpipe, a model of which has been placed in the Society's Repository.*

The following communication has been received from Mr. Maugham.

Adelaide Street Gallery,
May 12, 1835.

Sir,

Having had occasion to make a great number of experiments during the last two years with oxygen and hydrogen gases, and having found all the blowpipes for burning these gases in a mixed state, on an extensive scale, very inefficient for my purpose, I have been under the necessity of contriving an apparatus, easily manageable, and entirely free from danger. It would be useless to enter into even a slight history of the several contrivances that have already been laid before the public, from time to time, for burning these gases with

safety in a mixed state, as I am aware that the Society are fully conversant with all that might be adduced on the subject. I may, however, be allowed to state, that such contrivances, although highly ingenious, can, in reality, only be considered as toys in the hands of bold operators; for they are by no means calculated to answer any purpose by which the public can be benefited: for they cannot be trusted in the hands of ordinary manipulators. By means of the blowpipe which I herewith transmit, and which I trust you will submit to a full investigation and trial, these gases may be burnt for any length of time without any chance of explosion, as they are merely mixed in a very small quantity at once, the mixture taking place only as fast as the gases are consumed. The heat produced by this safe mode of burning the gases may undoubtedly be turned to an advantageous account by different mechanics, in fusing platinum and other refractory metals, by which means the expense now attendant upon the working of such metals would be considerably reduced. This mode of burning the gases will likewise be found eligible for producing a continuous and intense light through the medium of lime.

Professor Daniell's blowpipe is one that may be used with perfect safety; but, in my estimation, it labours under two great disadvantages, one of which is, that a sufficient degree of heat for many purposes cannot be obtained by it; the other is, that the great consumption, or rather waste, of gas, occasioned by its wide apertures, unfits it for use on an extensive scale. In this blowpipe, the gases are brought along separate tubes, one of these tubes traversing the interior of the larger one, so as to allow the gases to come in contact only just where they respectively escape into the atmosphere. As this is already in the hands of most chemists who have any thing to do with oxy-hydrogen blowpipes, instead of beginning with a mode of conveying the gases *ab origine* into the chamber which I am about to propose to the Society's consideration, I will describe a method of rendering it complete, and fully efficient for all the purposes to which the safe combustion of the gases can be applied. My motive for building upon Professor Daniell's blowpipe is merely to serve the economical purpose just alluded to; for it will presently be perceived, that it would have been quite as easy to have constructed one altogether different as to have undertaken to finish what that philosopher has begun.

Figs. 43 & 44, Plate VII. is a section of my blowpipe (part of the plain tube between the two arrows being omitted), with one of the nozles, hereafter described, screwed on the end

of it. At this part it will be seen that I have made a little addition to Professor Daniell's arrangement; for at the end of the tube along which the hydrogen passes, there is soldered on a piece of metal, perforated with eight holes *x*, and also with a larger central one, which receives the end of the pipe through which the oxygen passes: these smaller holes are not, as might at first be expected, to answer the purpose of wire-gauze, and to prevent explosion; but are merely to divide the stream of hydrogen, so as to render its mixture with the oxygen in the chamber into which the pipe opens more uniform. On the outside of the chamber is a support for a cylinder of lime, hereafter described. The nozle can be unscrewed, and the right-angled one, for fusing platinum, &c. put in its place. On the outside of the nozle is placed the support for the cylinder of lime.

The apparatus here shown is to be used as follows: It is to be screwed on at *c* to a gasometer, or to a tube leading from a gasometer, containing oxygen, and is to be connected, by means of a flexible tube screwed on at *d*, to another gasometer containing hydrogen,—a connecting joint will be necessary in making the union complete. The cock *e* regulates the quantity of oxygen, and is part of Professor Daniell's arrangement. The cock for regulating the quantity of hydrogen is to be attached to a flexible tube screwed in at *d*, which is not seen in the figure.

By having an elbow joint, a section of which is seen at *a*, fig. 45, and screwed in its place at *b*, *b*, a vertical position of the shaft of the blowpipe may be obtained, which will be found convenient for placing the lime-light in a parabolic reflector.

Fig. 46, shows the support for the lime without the nozle; *a* the rod on which a ball or cylinder of lime, &c. is to be placed,—the lime, &c. having, of course, a hole drilled through it, and turned perfectly cylindrical; *b* the ring which slips on the nozle of the blowpipe; *c* a screw for fixing the ring tight; *d* a small plate of metal on which the lime rests; *e* a screw for raising or lowering the lime, so as to expose a fresh surface of the earth as often as may be necessary. This does away with the necessity of employing a watch or clock-work motion, in ordinary experiments, with the light in question.

Fig. 47, is a section of one of the nozles of the blowpipe I beg to propose, showing a chamber *a* in which the gases mix previously to being burnt; *b* is a female screw for fitting it on the end of a Daniell's blowpipe. The jet *c* is of platinum. This nozle is bent at right angles, and, when fixed on the blowpipe, the jet for delivering the mixed

gases points perpendicularly downwards, and is intended for fusing platinum, and other metals difficult of fusion, the metal being supported on a proper rest. The oblique nozle shown in figure 43 is for experimenting with light by means of lime. It is bent at an angle of about 45°.

So long as the pressure at each gasometer is kept uniform, a proper mixture of the gases in the chamber will always be kept up after it has once been established by the regulating cocks, and an explosion cannot possibly take place. The gases are thus burnt certainly in a *mixed* state; but they are only mixed in a very trifling quantity, and only just before they escape from the blowpipe. It may here be necessary to observe, that in the several kinds of apparatus for burning the gases in a mixed state by the usual mode of mixing them. the mixture can never scarcely be what it precisely ought to be; for they are mixed in the proportions of two volumes of hydrogen and one volume of oxygen, no regard being had to the impurities which are always present in these gases as ordinarily obtained; which impurities are continually varying, not only in quantity, but in quality, as I have found by experience. By means of the apparatus just described, this inconvenience respecting mixture is entirely done away with; for the regulating cocks allow us at any instant, if the flame vary from what it ought to be, to bring it to its proper state. It is true, the mixture is made by guess; but a little experience will soon enable the operator to obtain at once the due proportions. If the hydrogen be in excess, a large bushy flame will be produced: if the oxygen be in excess, the flame will be extinguished: the flame produced by the proper mixture is so characteristic, that when once seen it is easily remembered. When the gases are thrown upon lime and the other earths, we shall have a flickering light, if the mixture of the gases be not properly adjusted; and this flickering we always have, when the gases are mixed in the manner to which I object.

Another addition to Mr. Daniell's apparatus is a connecting piece at *h h* fig. 44, which being slightly unscrewed will enable the jet to be placed in any direction in the plane of a circle whose diameter shall cut the blowpipe transversely at right angles.

With respect to the gasometers to be used with this apparatus, I have to observe, that on a small scale, those who are in possession of two gasometers, constructed on Mr. Pepys's principle, may obtain an equal pressure by employing one funnel, terminating in a branched tube attached to each gasometer, as shown in fig. 48. Under ordinary circumstances, an

assistant may pour water in the funnel *a*, and a *little* variation in the height of the column which will occur by this means, does not much affect the flame, and may be always counteracted by means of the regulating cocks. When there is convenience, the supply of water to the funnel may be regulated by a ball cock attached to a pipe passing from a cistern, the ball being placed in the water in the funnel. This mode of proceeding, however, only relates to the use of the gases on a small scale, as in the lecture-room, &c. When the gases are to be used more extensively, and applied to the purposes which I have already proposed, a better mode of obtaining equal pressure will very readily suggest itself. It is to be remembered that, in describing a blowpipe, I cannot be expected to say much respecting gasometers: all I wish is, that the pressure may be always uniform, and equal to that of a column of water, of at least twenty-four inches in height.

I trust you will at once be convinced that the mode above proposed is entirely free from danger. I may say with truth that I have burnt thousands of gallons of oxygen and hydrogen gases in this way, and have never yet met with the slightest accident.

In conclusion, I beg to observe, that I by no means wish to impress upon the Society that I have been the first to burn oxygen and hydrogen from separate vessels; but that this mode which I have proposed has been the result of my own experiments. I believe there is no blowpipe at present before the public for burning the gases on this principle. The mode of producing the light upon lime for the oxy-hydrogen microscopes by Cary, Cooper, and others, I was not acquainted with until long after I had obtained the light myself for the proprietors of the Adelaide Street Gallery; I always employed balls of lime, and through Messrs. Cooper and Cary, I learnt that cylinders of the same earth are decidedly better. The apparatus which I employed for a microscope commenced by Mr. Tully, is still at the Gallery, and is open to the inspection of any person who wishes to see it.

I am, Sir, &c. &c. &c.

W. MAUGHAM,
Lecturer on Chemistry,
Adelaide Street Gallery, and
Charing Cross Hospital.

A. Aikin, Esq.
Secretary, &c. &c.

P. S. If platinum be fused upon charcoal it will be brittle, and unfit for the purposes to which it is usually applied. I have no doubt it is converted into a *carburet*. I have tried several substances as a support for it, of which I find Stour-

bridge clay the best. Mr. Johnson, of Hatton Garden, has been kind enough to witness some experiments with the blowpipe, and he suggested the use of the above material as a support. He brought with him some bone-ash cupels, but they were readily fused. All substances, indeed, seemed to fuse under the intense heat produced by the combustion of the gases in question. I find the best mode of fusing platinum is to keep adding gradually to the fused mass small pieces of the metal. When an ounce or more has thus been acted upon, the metal will be in fusion at the surface, but will become solid at the bottom. We may thus go on welding or agglutinating the platinum to any extent. Before rolling or using the metal in any other way, be careful to cut off that end which was next the support, as this becomes incorporated with a portion of silica, which renders it unfit for working. By adhering to the principle laid down, by having gasometers sufficiently large (and their size may be increased to any extent with perfect safety), and by having the orifice whence the gases issue augmented to a considerable extent, I am convinced platinum may be fused in almost any quantity. I have succeeded in agglutinating more than half a pound of this metal by the process just described.

Mr. Maugham has lately informed the Editor of these *Annals* that Oxy-hydrogen Blowpipes constructed on his principle, have been brought to him imperfectly made; he therefore wishes any person who may purchase one of these instruments, to bring it to him at the Adelaide Gallery, where he may see it tried, and if there should be any defect in it, he will point it out, and the maker can then remedy it. The errors in the construction are in the orifices of the jets being too small or too large, and the same may be said of the orifices through which the hydrogen passes through the section x, to get to the mixing chamber. Those who prefer it, may have the blowpipe from Mr. Maugham himself.

XXXVII. *On the Voltaic Sustaining Battery. By* F. W. MULLINS ESQ. *M. P., F. S. S.; M. R. S. &c., in a letter to* W. STURGEON, ESQ. *Editor of the Annals of Electricity.*

Dear Sir,

In compliance with your request, I will endeavour to give you as correct a description as possible of my Voltaic sustaining Battery; together with the circumstances which led to its

discovery. About four years since I was engaged in a series of experiments upon electric light; and it is unnecessary to explain to you how much the inconstancy of action of a Wollaston battery was calculated to annoy as well as interfere with expected results in an enquiry of this nature; I at length saw that unless I could procure or construct a more perfect form of battery in which the power could be sustained for a few hours at the least, it would be quite useless to prosecute my researches any farther: it therefore became a matter of moment to devise some plan by which I should be able to obviate the defects of the batteries then in general use. About this period I happened to look into a work on natural philosophy* which I had just received, and in one of its treatises found an abstract from Thomson's Annals of Philosophy, vol. viii, 1—74, in the following words:—"The following singular fact has been noticed by Mr. Porrett:—'If a vessel be divided by a *membraneous partition* into two compartments of which the one is filled with water and the other contains but a very small quantity, and if the *positive* wire from a voltaic battery be inserted into the former, and the *negative* wire into the latter, the water will be impelled from the first compartment into the second, through the partition, and will at length rise to a higher level in the latter than in the former.' The author of the work in which this abstract is given, adds, that A. De la Rive, on repeating the experiments, arrived at the same results when he employed a fluid with a small conducting power; but that when a saline solution of sufficient strength was used, no such effect of impulsion was perceptible, but that the reality of such an effect established the existence of a mechanical force derived from the *current* of voltaic electricity." Here then was evidence, clear and incontrovertible, that membranic substance, moistened even with common water, conducted the electric current, but that with a saline or other good conducting solution the electricity no longer carried with it through the membrane a large proportion of the fluid:—in other words, that its mechanical force was neutralized by the increased powers of conduction of the fluid and membrane. It took but little time or trouble to construct a vessel with such a partition; I tried the experiment; it succeeded: and then it at once struck me that it would be but a little step forward to place a pair of plates, one in each compartment, in place of the positive and negative wires, by which means the electric current would find a ready passage, while the

* The library of natural philosophy, published under the direction of the society for the diffusion of useful knowledge.

grand object would be obtained of preventing positive elements finding their way to negative surfaces, and vice versa; which, according to Sir Humphrey Davy's opinion, is the chief cause of the decline of galvanic action: for he says: "that such transfer has an immediate influence in checking the farther progress of the galvanic action: and arrests it completely when it has proceeded to a certain extent." I may here observe in passing that I was not a little surprised to find it asserted in the last edition of Brande's Manual of Chemistry, that Mr. Daniell had discovered the cause of the gradual decline of action, in batteries of the usual construction; for I thought that every human being who knew any thing of the science was very well aware that Sir Humphrey Davy and other eminent men of his time understood as well as any person now living, the true causes of diminished power in voltaic batteries. But to return to my experiment. On completing the circuit I found that, according to my expectation, the battery worked with undiminished power for a considerable time, and that at length I had obtained an instrument of research such as I had long been desiring; thus, that simple arrangement which, in Mr. Porrett's hands, had been applied to the demonstration of a certain mechanical force of electricity, became, in my hands, the ground-work of the sustaining battery, which, after various experiments, I have since brought to greater perfection. Before I proceed to give a description of the battery in its present improved state, it may be well to observe that the one I first employed was square, and fitted up in a trough of baked wood. I found, however, this mode of arrangement troublesome, as it was exceedingly difficult to fix the membrane so as to prevent leakage. I therefore adopted the cylinder form, not only because it enabled me to employ bladder for the interposed membrane, but because earthenware pots or crocks of this figure were easily procurable, and well adapted for insulation: and from the particular principle applied in the construction of my intensity sustaining battery, this form became a manifest advantage, as I shall explain in another part of this paper. I would add, that I was induced to employ sulphate of copper in solution, from the results of some experiments on the action of electro-negative surfaces on metallic solutions; and having employed this salt as well as many others in succession, it struck me that the action of the battery was improved whenever I charged the cell containing the copper with this solution. In a paper which I published in the Philosophical Magazine, I have adverted to the peculiar action of the sulphate in the voltaic circuit, and therefore will not lengthen this communication by more than

a few remarks upon that subject. The battery I generally use for my own purposes consists of ten pots each, containing a single arrangement, and constructed in the following manner. Close to the inner surface of an earthenware pot four inches high and two and a half wide, is fitted a cylinder of zinc, the depth of which is about a third of the depth of the pot: a small piece of the zinc, about half an inch wide, rises above the level of the remainder, about an inch, and to this is soldered a narrow ribbon of copper which rises to the top of the pot and projects over it about five inches, for the purpose of communicating with a mercurial cup; within this cylinder of zinc, and as close to its surface as possible, stands a copper vessel, the height of which equals the depth of the pot. This vessel is two and a quarter inches wide, and has either a wooden or copper bottom water-tight. Round the upper edge of this cylinder and external to it is soldered a rim of copper about a quarter of an inch wide, on the outside of which is worked a groove all round: in the upper surface of this rim are two holes as large as it will allow, for the purpose of drawing off the charge or supplying it. The copper cylinder thus constructed, is placed upon a flat circle of cork, open in the centre, and projecting as much from the outer surface of the copper below as the rim does above; this cork is wound round with strips of membrane, and a thin calf's or pig's bladder previously steeped in tepid water, is drawn over the cylinder, the use of the cork being to preserve the membrane from contact with the copper; the bladder is drawn tight, and fastened by a string round the groove in the rim before described. A narrow band of copper is soldered to the upper edge of the cylinder, and the battery is now fit for use. In charging it, I use two solutions; that in contact with the zinc, being one part of a saturated solution of muriate of ammonia to five of water; and that in contact with the copper, a saturated solution of sulphate of copper. I say saturated, not because it is requisite for the production of the maximum effect, but because it is convenient, as I will presently show. In charging the battery, it must be borne in mind that the solution in contact with the zinc should never be more than sufficient to attain a level with the upper edge of the zinc, for it is a matter of some importance to keep that part of the zinc which is soldered to the copper band free from local action. The other solution may be allowed to rise to about two thirds of the height of the copper cylinder, for the larger the charge of this solution, and the greater the quantity of the salt dissolved therein,—not the greater the power developed, as some persons who have written lately on the subject, would have us suppose,—but the

longer will the power, which a smaller charge unsaturated can equally develop, be sustained. It is not, therefore, because the fluid contains a very large quantity of the metallic salt in solution that active effects are produced, but because there is a certain quantity present, which suffices to produce in a given moment of time, that unit of metallic precipitation which is proportional to the surface of the metals employed, or rather to the various attractions called into play in the circuit. And that this quantity is exceedingly small is proved by the fact of the power continuing undiminished in the slightest degree, until the solution becomes nearly colourless; and farther, by another fact, that if to this nearly colourless solution you add about six drops of the saturated solution, the original power of the battery is instantly restored, and continues for some time. It may be asked how I manage to keep the battery in action for a long period, unless I keep crystals in solution. I meet the difficulty by drawing off the original charge of sulphate, by means of a syphon or a common syringe, to which is fitted a long and narrow pipe which enters the holes before-mentioned in the rim of copper, and by means of a small funnel renewing it. This may be done without interfering in the least with the action of the battery. It may be well here to observe that the other solution does not require renewal: however, as there are many who may prefer keeping crystals in solution to the trouble of drawing off and renewing the solution; for these I have designed a very simple mode of gratifying their fancies or their prejudices and at a very trifling additional expense. It is a form I sometimes use myself, and is nothing more than the soldering a copper shelf or plate within the cylinder, and about an inch and a half from its upper edge, and making six small holes on a level with the shelf to communicate with the external surface of the cylinder.* As soon then as it appears that the original solution has nearly lost its copper, a very few crystals may be dropped on the shelf, which will be carried in solution to the external surface of the copper. The battery exhibited at the Royal Institution on the 3d of June, 1836, was somewhat different from that described here; the copper cylinder

* In Plate VII. fig. 49, will be found a plan of Mr. Mullins's voltaic sustaining battery, as described above. A A, is a cylinder of rolled zinc made to fit the pot; B B, in dotted lines, is the membrane drawn round the bottom and enclosing the circular piece of cork i; e e, is the circular rim at top to which the membrane is fastened; c c, is the copper cylinder with two of the six holes k k, visible on a level with the shelf D; G and H are copper ribbons proceeding from the copper and zinc.

having a number of holes at regular distances all round and near the bottom, for the purpose of admitting the fluid to internal as well as external contact. However, this plan did not prove to be any advantage; on the contrary, it led to great waste of the solution, therefore I gave it up; and I find that the simple form described, either with or without the shelf, gives immense power and in a very small compass, which is one great advantage peculiar to this battery. Another advantage is the bringing the zinc surface so very close to that of the copper, which, though Professor Daniell appears to entertain a contrary opinion, I hold, from the result of accurate experiments, to be necessary for the full development of voltaic power. A third advantage is the very small quantities of fluid used, the charge not being more than about three fluid ounces of the sulphate, and two of the muriate; and a fourth advantage is the nature of both solutions which is such as to prevent their producing any of the inconveniences of acids. I have already stated that I use but a very small proportion of zinc in my batteries. This rule I have not adopted without convincing myself by the results of an extensive range of experiments that equal surfaces of zinc and copper developed no greater power than a much smaller surface of the former metal with the original surface of the latter. Marianini held this opinion though in an exaggerated degree, and a single experiment out of many will prove it true. Take a cylinder of zinc fitted to my battery, and of equal surface with the copper; charge the battery and connect it with a magnetic voltameter; note the deflection—I mean the *steady one*; remove the larger zinc, introduce one an inch less; again note the deflection; it will be the same. Remove this, and replace it by a smaller, and so on; still noting the deflection, until you bring the cylinder to a surface about a fourth of that of the copper. The deflection will still be unaltered. Try any other experiment of comparison you please and the results will be found to correspond; but now reduce the zinc to the size of a quarter inch band and you will find the power considerably diminished. This proves that there is a certain proportion of zinc which is capable of developing the same quantity of electricity that may be produced by equal surfaces; and this being the case, I apprehend that nobody will, in future, submit an extra and useless quantity of zinc to waste and local action, when he can obtain the same results from more limited means. In a paper written for the Philosophical Magazine I have entered more at length into this subject, and given the results of other experiments. I can only say that if they are not satisfactory, it is easy for those

who doubt to ascertain the facts by experimenting for themselves.

My intensity sustaining battery is constructed as follows:— I have first, as in the quantity battery, a shallow cylinder of zinc within, and close to the internal surface of the earthenware pot, next the copper cylinder as before; but instead of letting the inside of this cylinder go for nothing, the internal surface of the copper is lined with very thin *caoutchouc* for insulation; then comes another small cylinder of zinc; then a copper one, lined as the last; then a zinc; and lastly a copper: each copper of course enveloped in membrane. This battery is one of extraordinary power in decompositions and other effects of intensity, which, in my opinion depends upon a new principle which is developed in this mode of construction and arrangement, that is, the *restricting* the electric current to gradually *diminishing metallic surfaces* as it advances, so that as the quantity accumulates the conducting surfaces are *reduced*; and of course a much higher degree of intensity is a necessary consequence. As I mean to present a paper to the Royal Society on this matter, I will not go farther into the subject, and will now conclude this communication by intimating my intention of giving you for your succeeding number the results of some experiments on the electrical causes of chemical action.

I am,

Dear sir, &c. &c.

F. W. MULLINS.

February 2, 1837.

P.S. I nearly forgot to mention that I have found white silk of a certain substance to answer fully as well as the membrane, and indeed better, for it lasts so much longer; but as some silks fail, I do not wish, as yet, to recommend it for general use until I have completed some experiments which I have been for some time making on this subject. It may be asked whether the silk does not permit the two solutions to intermix? This is the singular fact in its employment that it does not. I have kept a battery in action for several days with only silk interposed, and the muriate solution never exhibited the slightest trace of copper, which a thin membrane occasionally does.

J. W. M.

XXXVIII. *On the luminous properties of Chloride of Calcium, by* CHARLES TOMLINSON, ESQ.*

When chloride of lime is subjected to a red heat in a Hessian crucible, it undergoes the watery fusion and is gradually decomposed. Chlorine is at first evolved, then oxygen; and chloride of calcium remains in the crucible. This substance, when thus treated, emits a pale green phosphorescent light, visible in the dark, and was formerly called on this account, *Phosphorus of Homberg*, this chemist being the first to record the fact.

But when the chloride of lime is completely melted in its water of crystallization, if the crucible be removed to a darkened room and allowed to cool gradually, not only does the phosphorescent light remain for many minutes, but large and distinct electric sparks and coruscations are visible, accompanied by a crackling noise due probably to the sudden contraction of the mass, or to its crystallization.

These phenomena can also be seen in the dark by dipping a glass rod into the fluid, a large portion of which concretes around it, and can be withdrawn with the rod.

When the contents of the crucible have cooled down, and all phosphorescence has disappeared, the sparks can be obtained most admirably by striking the chloride with a sharp instrument of any material; by simply scratching it; or by snapping a piece asunder; or by percussion with a hammer; and both sparks and phosphorescence can be obtained by grinding the substance in a mortar, and the luminous effects remain for a few seconds after the grinding is discontinued.

The colour and intensity of the sparks thus produced seem to vary with the degree of mechanical force employed; thus, a gentle scratch produces green sparks; a firm scratch, yellow green; a blow with a sharp instrument, yellow; snapping a piece asunder also affords a yellow spark, and a smart blow with a hammer affords a spark of an orange colour.

All these effects are best seen with the newly fused chloride; it absorbs water by exposure to the air and its electrical properties gradually decline; I have, however, obtained the sparks from it after a few days' exposure, but they were wanting in the brilliancy which characterize those obtained from the substance when newly fused.

* Communicated by the Author.

The term *phosphorescence* seems to be a very bad one and calculated to mislead; since it is highly probable that all recorded instances of phosphorescent minerals, &c. (except, of course, phosphorus itself and its compounds) are due to electricity. It has been shown by Dessaignes that metallic bodies are capable of electric excitation by the simple processes of treating and cooling. Morgan has shown that there is no fluid nor solid but may be rendered luminous by the transmission of an electrical discharge through its substance; and that the difficulty of producing this appearance in all bodies increases as the conducting power of the body decreases. Skrimshire has given a long list of minerals, in which calcarious bodies occupy a conspicuous part, which become phosphorescent by the electrical discharge; and Brewster also has a long list of mineral substances which become phosphorescent by heat. Now the appearance of the hot chloride of calcium is similar to that of white sugar, immediately after an electrical discharge has been transmitted through it. All calcarious substances present this appearance in a high degree when similarly treated, and the exciting cause is probably the same; for whether we employ the electrical battery or the hot crucible, heat is communicated to the substance, and heat is favourable to electrical excitation. A piece of lump sugar, mica, &c. when suddenly snapped asunder, and cotton cloth when suddenly torn, show the electric spark; so also does the fracture of the chloride of calcium. An enquiry, therefore, seems to be wanting into the connexion between phosphorescence and electricity since it is manifest that the appearances attending the slow combustion of phosphorus, and the light emitted by certain minerals when heated, &c. though similar in appearance are due to very different causes.

Salisbury,
March 4, 1837.

XXXIX. *On a simple Voltaic Battery, in a letter to* WILLIAM STURGEON, ESQ., *&c. &c.*

Dear Sir,

On taking a retrospective view of the different arrangements of the galvanic apparatus, from the pile of Volta to the battery of Professor Daniells, the following points will, I think, be tacitly agreed to by all experimenters.

1st.—Their complicated arrangement.

2d.—The limited duration of their action.

3d.—Their original expense and cost of repair when out of order.

Of those prior to Professor Daniells I need not refer to;* the latter, although possessing a decided superiority over the old arrangement, with regard to its sustaining power, is yet very far from being either a convenient or an economical battery.

The arrangement of F. W. Mullins, Esq. M. P. is again superior to the one preceding, but, at the same time, it is equally as expensive.†

The miniature arrangement of the Rev. J. Shillibeer, although very powerful for its size, labours under the disadvantages of limited action and expensive outlay; and the cost of repair equally so.

As all ranks and grades of men engaged in scientific pursuits are, at present, busy with the examination of the phenomena exhibited, by that all absorbing subject, Electro Magnetism (which by the publication of your interesting Annals is not likely to share the fate of its twin sister, Electricity, commonly so called), the following arrangement of an economical and powerful sustaining battery may, perhaps, prove interesting to some of your readers.

A piece of thin sheet copper is coiled up into the form of a cylinder, and retained in that position by fine copper wire. The size I usually employ is that of 4 inches by 2½; it is then to be placed in a small bladder, which is secured round the same by pack thread, leaving the top open, the membrane forming the bottom of the cylinder; a piece of zinc is coiled

* You were, I believe, the first to employ cylinders of the metals, as a galvanic arrangement; and, therefore, the battery to be hereafter described is simply a modification of your original plan. This, with the one invented by E. M. Clarke, where a double plate of zinc is made to dip into two copper troughs, soldered together, are certainly the most economical arrangements upon the old plan; the zinc being easily replaced, and for the mere cost of the metal.

G. H. B.

The battery which Mr. Bachhoffner here alludes to has been laid aside for ten or more years; not only because of its expense, but because of the brazing necessary in its construction, causing considerable local voltaic action, and of the loss of one surface of the copper. Since this battery was out of use I have employed cylindric scrolls of copper and zinc, loose at the edges, and without any soldering exposed to the exciting solution. To prevent contact, and gain proximity, the zinc scroll is covered with a strip of cotton cloth (calico) and placed inside the copper. The whole is placed in a white porcelain jar. W. S.

† Mr. Bachhoffner, of course, cannot allude to the battery described by Mr. Mullins in this number. Edit.

up in a similar manner, having previously soldered a copper wire to each to form the connexion, and the battery is completed. To excite it, place it in any convenient vessel; I usually employ a jellypot, and pour into the copper cylinder a saturated solution of the sulphate or any other salt of copper, and outside the same and in contact with the zinc, must be placed another solution, it matters but little of what nature: one of common salt I find to be as good as any that I have tried, and it has the advantage of being always at hand and costs but little, the latter appearing to act as a conductor only; if the battery is required to be kept in action for two or three days, a few crystals of the salt of copper must be placed in the solution of the same.

From this rough statement, it is evident how cheaply an efficient battery may be obtained, and at how little cost it may be repaired when the zinc is destroyed. With an arrangement of six of these batteries, water is rapidly decomposed, metallic wires fused, and brilliant combustion of the charcoal points is obtained.*

Should you consider the above of sufficient importance your insertion of the same will oblige,

Dear Sir, yours faithfully,
GEO. H. BACHHOFFNER.

13, *Aberdeen Place,*
Maida Hill.

P.S. I cannot help thinking that the action of the metallic salt in the several arrangements has been much under-rated; neither can I agree to its action being limited to the absorption of the hydrogen, and its conducting power only, I am rather inclined to consider, the superior action and quantity of Electricity developed by these batteries to depend upon, the decomposition of the metallic salt, or rather its development, is the result of the decomposition continually going on, in the above solution. I am now engaged in several experiments on the subject, the result of which I shall be most happy to communicate.

XL. *A letter to* W. Sturgeon, Esq., *&c. &c.*

Dear Sir,

As almost every scientific enquirer is at present directing his attention to the construction of voltaic batteries, and as, in my opinion, there is no correct measures of their relative

* A series of twenty of these are now in action at Mr. E. M. Clarke's Establishment, and may be seen at any time, by application to him.

merits, I beg leave to submit to the notice of your very numerous readers two instruments I have constructed for that purpose.

Fig. 50, Plate VII, the thermo-electrometer, A, a piece of platina wire, say the $\frac{1}{50}$ of an inch diameter, secured at B by a pair of forceps; at the other end C, is also a pair of forceps, fixed in the mahogany foot and insulated by a piece of ivory, or glass. D, a brass upright, having a binding screw to press on the sliding scale E. An index is fixed at F to read the scale from. The scale can be elongated by additional pieces, as at G. It is almost needless to add that the length of wire made red in darkness, or burned, is the measurer of power.

Fig. 51, the electro-gasometer, A, a glass vessel having a similar arrangement at B of the platina wires, as described at page 150, of No. 2, of your Annals; C, a graduated glass tube, sealed at top and standing over the platina plates; D, a circular piece of wood, into which A fits loosely. In the centre is a divided chamber, which by wires are in connexion with the mercury cups, marked copper and zinc. To use this instrument.—Having filled the vessel A with water up to A, then grasp it in your hand so that your finger will stop the air hole F, invert it and the water will fill C; having put mercury into each side of the divided chamber E, and also in the mercury cups, place A in D, so that the copper points 1, 2, may dip in each side of the divisions in E. The power of the battery is obtained by noting the amount of gases given off in a certain time.

I constructed this instrument from having seen a very imperfect one at the Royal Institution made out of a two-necked bottle with the platina electrodes passing through two holes drilled in the graduated glass tube.

Your's sincerely,
E. M. CLARKE,
Magnetician.

Laboratory of Science,
11, *Lowther Arcade*,
(*Late of Agar St.*)
February 28th. 1837.

XLI. *An account of various arrangements of the Voltaic apparatus, and Remarks on the limited extent of Metallic surface, sufficient for electrical excitation; with a few observations on electro-chemistry, in a letter to* WILLIAM STURGEON, ESQ., *&c. &c.*

My Dear Sir,

Having been engaged, occasionally, for some weeks past, in experimenting upon the voltaic apparatus, with a view to ascertain the best form and arrangement of the metals, &c,

I beg leave to submit to your notice the following results, premising that I neither lay claim to any originality in my mode of investigation, nor do I pretend to attach any importance to the results obtained, farther than in as far as they appear to agree with, or to differ from, the results which have been obtained by other enquirers; and as they tend to warrant the inferences drawn from them. I more particularly refer to the disproportionate amount of effect between the transient and the permanent deflections of the galvanoscope, to which you have yourself directed attention in your excellent Annals, and to the apparent inutility of employing metals presenting great extent of surface.

One of my earliest experiments, (made in Sept. last), is the following:—In a glass vessel containing a saturated solution of the sulphate of copper, I placed a glass tube, in which was inserted a coil of copper wire. In this tube was inserted another, enclosing a zinc wire. This was my battery, with which I obtained a permanent deflection of the magnetic needle of 50°. The needle employed in all my experiments is not astatic. The wire passing above and beneath it contains thirty-one convolutions. The needle is not suspended, but moves on the point of a common pin.

Another form of apparatus is the following:—A small piece of wood, cut so as to receive a small zinc plate; I crossed this plate diagonally with merely copper wires. The size of the zinc plate is only two inches by one and a half inch; thickness of the wires one eighteenth of an inch, crossed only twice in one direction, and three times in the other. With this simple battery, the following were the results.

Exp.	Exciting liquid.	Transient deflect.	Permanent deflect.
1	New River water	not noted	7°
2	Moistened Car. of soda	40°	14°·5
3	Solution Sulph. of zinc	27°·5	9°·25
4	Benzoic acid	25°	10°
5	Tartaric acid	35°	13°·5
6	Boracic acid	70°	15°·5

These experiments were made before the conducting wire was soldered to the zinc. After having soldered it, a solution of sulphate of copper being added, there were four successive transient deflections of 180°, and the permanent deflection was 45°·9. And with a solution of the sulphate of iron, the transient deflection was 80°; but the permanent deflection only 11°·5.

In these experiments, the disproportion between the transient and the permanent deflections demand attention. Indeed the results would almost appear to warrant the inference that the electric fluid is modified in some degree by every substance in which it exists, and that it is liberated from different substances with various degrees of force, during a change of state in those substances. But it would be premature in this place to offer any further observations, until an extensive series of experiments shall have been gone through on this point.

Another subject of enquiry is, how far extent of metallic surface influences electrical excitement. And here I would venture to remark, judging from the results of my own experiments, that much more seems to depend upon the form and arrangement of the metals, and the disproportionate extent of the surface of one of the metals, as compared with that of the other, than on mere extent of surface. And although I have not made any direct experiments on this particular, yet I may infer from those of others and my own, that either a large zinc and a small copper plate, or a large copper plate and a small one of zinc, will form a powerful battery. The latter mode is adapted by Mr. Mullins, and by Professor Daniell. I was induced to try the effect of an arrangement the reverse of theirs, by reasoning thus. In the excitation of dry electricity, a very small cushion is sufficient to throw the whole surface of a large glass cylinder into a positive state. Then, as copper is the negative metal, when employed with zinc, analogy led me to try the effects produced by a comparatively large surface of zinc, in proportion to that of the copper, and the results have fully answered my expectation. I took a cylinder of zinc only two inches long, and half an inch in internal diameter, and inserted by means of a cork, a copper wire one sixteenth of an inch in thickness. This small battery with a solution of sulphate of copper kept the apparatus of Dr. Ritchie rotating for eight hours, and it only then ceased because there was no provision for keeping the solution at its point of saturation.

Since the foregoing was written, I have continued the experiments, chiefly with a view of attempting to ascertain how far extent of surface is essential to the development of a quantity of electricity, and in reference to the relative extent of surface of the dissimilar metals employed in the battery. And from the results, I find greater reason to adhere to the opinion that one of the metals should present a much greater extent of surface than the other. And further, when zinc and copper are used, that the surface of the former should greatly exceed that of the latter. The experiment before

alluded to may be adduced in support of this opinion, and also the following, which I have since tried. I formed a helical coil of zinc wire of one sixteenth of an inch in thickness; I inserted, by means of a cork, a piece of copper wire one eighth of an inch thick. The whole length of the battery, exclusive of the terminations of the wires at top, was only two and a quarter inches; and the diameter internally of the cylinder formed by the coils, only one quarter of an inch; yet this was sufficient to rotate both Dr. Faraday's and Dr. Ritchie's apparatus, and to give a very good spark. The liquid used was a solution of the sulphate of copper. From these and similar experiments we are led to infer that there is a wide difference, indeed, between the quantity of electricity developed, and the amount rendered available for experimental purposes, and that the battery which is so constructed as to transmit *all* the electric fluid which is excited is undoubtedly the best. Before, however, we can succeed in the construction of such a battery, we must first make ourselves acquainted with the source of the electric fluid in a voltaic arrangement. Second, with what share each of the metals has in the effect produced. And third, deduce what ought to be the arrangement, and the relative size of the metals, so that each may perform its duty in the most effective manner.

1. What is the source of the electric fluid, in a voltaic arrangement? There are numerous opinions on this point, which it is not necessary to enumerate. All, however, or at least the great majority of them, agree in one respect, viz.: that the electricity is first set in motion by chemical action. It ill-becomes so humble an enquirer as myself to advance an opinion diametrically opposed to that which is generally received by most of the philosophers of the day. But it is an opinion which has not been formed hastily, or without a patient consideration of all the phenomena attending electrical excitation; and I presume to hope also one that will be found sufficient to explain those phenomena, as likewise those attendant upon what is termed chemical action. First then, it would appear that the first effect which is produced in a voltaic battery, is this:—(supposing the metals to consist of copper and zinc) Zinc, in common with other bodies, possesses a certain quantity of the electric fluid. The copper is a better conductor. By means of the conducting medium interposed between these metals, a stream of electricity moves from the zinc to the copper. Chemical action is a consequence, by no means a cause, of this current, and it may be thus explained. As a portion of the fluid leaves an atom of zinc, that atom enters into combination with oxygen, obtained from

the solution. This, again, forms a source of electricity; for, the oxygen in passing into combination with the zinc, also evolves its latent electric fluid. Hence decomposition also takes place in the solution. Thus we see the copper precipitated from its solution. Why then, in those batteries wherein a membrane is not employed, is the metallic copper precipitated upon the zinc, in place of being carried round by the current, and deposited upon the copper? Because, owing to its great conducting power, and in consequence of its being liberated in an extremely minute state of division, it is strongly attracted towards the positive metal. But this its natural tendency, being prevented by the interposed membrane, it is constrained to attach itself to the copper plate From hence it would appear that chemical action is not the cause of electrical excitement, but that what is commonly termed the cause is, in reality, the effect. It will be said that this amounts to an assertion that electricity is the cause of cohesion, which, indeed, it probably is, as we may infer from the results of numerous experiments; amongst others the following. If we break a piece of loaf sugar in the dark, electricity is developed, and a flash of light is distinctly seen. If we break a stick of sealing wax, electricity is also excited. Now, of course, upon destroying the cohesion between the particles of bodies, we ought to look for the liberation of the agent concerned in the production of that attraction. On breaking the sugar, and the sealing wax, what is liberated? Electricity; and it is given out merely at the place of fracture. Hence it follows, that whenever circumstances favour the transmission of electricity from any substance, the cohesive attraction between the particles of that substance will be destroyed, and it will be decomposed more or less rapidly, according as adventitious circumstances concur in promoting that effect with facility, or gradually. Now, let us return to the battery. As before stated, the transmission of the fluid from the zinc to the copper is favoured by the interposition of a conducting liquid. As the electricity leaves the zinc, the cohesive attraction of its particles is destroyed, in consequence of the cessation of the cause of such attraction; decomposition takes place, and the zinc enters into combination with oxygen. Now, here it will be seen that every circumstance tends to favour the display of the electro-motive disposition of the subtle fluid. The presence of a conducting liquid affords it a ready passage, the proximity also of an electro-negative element, (oxygen) which is of course attracted strongly towards the positive metal, tends also to weaken the cohesion of the particles of the metal. It is not meant to be denied that other sources of electricity are

not to be met with in the decomposition of the conducting liquid; but it is contended that these are consequent upon the first action, which is the transmission of the electric fluid, as above explained, from the zinc to the copper. Nor is there any thing in this theory which is discordant with observed facts or phenomena, either in electricity or in electro-chemistry. On the contrary, it seems to furnish us with a ready solution of the cause of the decomposition of compound bodies, and the formation of new substances from their elements. Ex. gr., If we take a solution of the nitrate of the oxide of silver, and insert a zinc wire in the solution, a precipitation of the metallic silver takes place. What is the explanation of this? We are told that it is owing to chemical action. But what is chemical action? Some change must have taken place, before such action commenced. Why, yes, we are again told, it is affinity that influences such action. Affinity! why, it is a mere word, explained, indeed, in the statement that different bodies manifest different degrees of attraction towards each other, &c., &c. But in what consists this attraction? Where does it reside, and by what material agent is it effected? Do we not at once perceive, that if this question is not answered, we are merely stating that one atom attracts another, because it does so attract? Now, let us attempt to explain the action, not chemical, but electrical, in the precipitation of the silver from its solution in the above experiment. Here also, we have a conducting liquid, which, conveying from the superficial atoms of the zinc, a portion of their electric fluid, by virtue of which they were held together, thus destroys the cohesive attraction, and a mutual decomposition results between the liquid and the zinc. And thus may be explained all those varied actions that are termed chemical. They all would seem to result from a diminution or destruction of the cohesive attraction between the particles of a body, under circumstances favourable to the transmission of electricity from that body. Having thus, then, endeavoured to trace the electric fluid developed in the voltaic apparatus to its source, we now proceed to enquire,

2. What share each of the metals has in the production of the effects? It has already been stated that the fluid moves from the zinc to the copper. The latter, then, may be considered simply in relation to the zinc, as a conductor. And this will afford us with an explanation, why, when the circuit is formed, the fluid moves along the conducting or transmitting wires. Because the copper is a more perfect conductor than the liquid. Such then being the case we are led to enquire,

3. What ought to be the arrangement, and the relative

size of the metals entering into the formation of a voltaic battery. And first, it would appear from the results of the experiments before alluded to, that a comparatively small extent of copper surface is quite sufficient; and that there is nothing gained by either the zinc or the copper exposing surfaces of such extent as those which have, until very recently, been employed in the construction of batteries. Perhaps the exact proportion might be arrived at in this way. If we ascertained the difference in the degree of oxidability between the copper and the zinc, and the difference in their power of conducting the fluid, the difference between the sums of those powers might lead us to approximate towards the proper proportions between the two metals. Of course I am speaking throughout this paper, of single batteries. Another point to be attended to is the proximity of the metals. They should be as close as possible, so that the electricity in motion may be transmitted to the copper, and along its conducting wire at the moment of its liberation; for it seems essential to the due effect of single batteries, that the fluid should be afforded every facility for free motion; this free motion constituting what has been generally termed quantity, which it certainly is, but not in the sense it has been employed. From these observations it follows that the extent of copper surface may be either too limited or too great. If it is too small, the current from the zinc would, in all probability, be proportioned to it, so that we should not gain all we might with the same extent of zinc surface, with a negative metal of greater size. If, on the contrary, the copper surface is too extended, the disproportionate size of the receiving surface, compared with the transmitting surface, would have the effect of so dispersing the electrical atoms, as materially to interfere with the effects which ought to be produced by single batteries. It would, in effect, be a waste of a great portion of the electric fluid developed, inasmuch as owing to its being spread over so large a surface in one of the metals constituting the battery, a quantity might not be conducted in a given time, sufficient to the production of the effects expected from it.

In endeavouring to account for the *modus operandi* of the voltaic apparatus, it first occurred to me that the electricity resulted from the decomposition of the exciting liquid. But the question immediately occurred, what is the cause of such decomposition? In attempting to answer this question, it became evident that I was commencing with a mere effect.

The same difficulty arose upon the supposition that the electricity was disengaged in consequence of the oxidation of the zinc. There must have been a previous action separating

the oxygen from the liquid, and weakening the cohesion of the zinc, thus placing the two bodies under favourable circumstances for combination. I concluded that the first action, therefore, could not be attributed to chemical action, but must be sought for in the cause of such action. Now, cohesion, being the great antagonist to chemical combination, it appeared that some action of that which is the cause of cohesion must first take place. And thus, as before stated, I was led to believe that the first action was the transmission of electricity from the zinc to the copper, and that all the other actions were consequent. Now, many facts tend to show that the electric fluid is the cause of cohesion. Besides those before adduced, may be added the fact that heat diminishes the cohesive force, and it is well known that electricity is disengaged, when heat thus acts; which is particularly exemplified by the metals.*

I have thus given you the substance of my notions as briefly as possible, and shall enter more minutely into the subject in my next communication. If there is any plausibility in the ideas I have ventured to advance, the importance of their application to explain all those actions which are termed chemical, will be evident.

I remain, my dear Sir,
Your's faithfully,
WILLIAM LEITHEAD.

P. S. I have to apologize to you for the hurried manner in which the foregoing paper has been written. Indeed, it was intended merely as the rough outline of a paper; but since I have not now time to alter it, I am under the necessity of forwarding it to you in its crude and undigested state.

I beg leave to subjoin a hurried account of the way in which I was led, step by step, to the opinions I have ventured to advance; promising, in a future paper, to enter into a more detailed account of the experiments, and of the inferences drawn from their results.

* Indeed it would appear that whenever a body is heated, it parts with electricity; and *vice versa*, when a body is giving out caloric, it absorbs electricity. Hence, it may be inferred that the natural quantity of electricity of a body is continually varying with the temperature of the air, or in consequence of other sources of heat, affecting its calorific state. The importance of this law in a meteorological point of view is evident. But more of this hereafter.

XLII. *Description of a new arrangement of the Voltaic Battery and Pole Director, by the* Rev. John Shillibeer, *A. M. in a letter to* William Sturgeon, Esq., *&c. &c.*

Grammar School, Oundle,
Sir, March 9, 1837.

In the course of the last winter when I was preparing a few lectures on experimental philosophy for the amusement of my scholars, I was struck by the complicated arrangement of the voltaic battery, and the difficulty which frequently occurred from the number of connexions, to get all the wires into so perfect a contact as to ensure success to the experiment. A little reflection on the subject convinced me that the voltaic pile might be so constructed as to be at once materially lessened in size, and improved in simplicity and power. At the same time, a movement might be appended to it which would at pleasure change the current of electricity, and command, of itself, that reversal of the rotatory motion which hitherto has been effected, only by help of the electrepeter; an ingenious apparatus, constructed by Mr. Clarke, of the Lowther Arcade, but too complex and scientific for bringing, at one glance, to the comprehension of a juvenile audience, the circuitous route taken by the electric fluid.

Finding on trial that a single piece of zinc (surface three inches) surrounded by a gut or membrane, when placed in a copper vessel containing a solution of sulphate of copper, could be soon erected, and that a rotatory motion might be kept up during pleasure, I formed the apparatus about to be described, which consists of a well-made copper trough, about three inches deep and two and a quarter wide, divided into compartments, according to the number of zinc plates employed, (vide fig. 58, Plate VIII.) which represents a section of the copper trough that receives the plates. These plates are soldered firmly to a copper bar, and by the aid of a screw are fastened to a piece of hard wood answering for a cover to the trough, which, with the zinc plates and movement for directing the course of the electric current, may be seen by a reference to fig. 57. In a groove cut out in the cover, on each side the screw, is fitted a copper slide; and these slides are joined by an elbow to a piece of ivory forming a handle, by which, passing immediately over the screw, each wing of the slide may be brought readily into contact with the copper or zinc. The use of this movement, or pole director, I will endeavour to make apparent.

In fig. 57, let A represent a section of the wood cover with a groove for the slide to stand in. B, the screw in connexion

with the zinc. C, C. the opposite sides of the copper trough. D d, E e, the two wings of the slide. Now let the wing D d be in contact with C, and E e with B, it is evident that the stream of electricity is going out from the wire in connexion with the wing D d, and is returning by the wire appended to the wing E e, into the zinc plates, *via* B. Shift the slide so that E e be in connexion with C, and D d with B, the stream will be reversed, making its exit by E e, and returning by D d to the zinc *via* B.

The zinc plates should be covered tightly with a membrane, so as to prevent any possible precipitation of the sulphate of copper upon them. When used for suspending a weight, the membrane should be slipped off and instead of the sulphate of copper, diluted nitrous acid should be used.

This, Sir, is the history of the voltaic apparatus I have constructed, and which has been submitted to public notice more in compliance with the wishes of friends than from any personal inclination. It certainly may lay claim to these advantages, viz. that it is small in size, not very susceptible of injury, and easily repaired: moreover it requires but little food, and with that little will perform a good honest day's work, whether it be to suspend a weight, bring platina wire into red heat, or keep up a powerful rotatory motion.* I can only add that if you think this short account of it likely to be acceptable to the readers of your very useful and interesting Annals, you are welcome to use it in the way you shall think most proper. I cannot but feel gratified at the success that has hitherto attended the career of my little instrument; and very glad shall I be if it may lead to other and more important improvements in aid of a science which, let us hope, may be ultimately applied to purposes of solid benefit to all mankind.

I have the honour to be, Sir,

Your's very faithfully,

JOHN SHILLIBEER.

XLIII. *On Mr. Fox's (alleged) conversion of copper pyrites into purple, and vitreous copper. By* W. J. HENWOOD, ESQ. *F. G. S. Hon. M. Y. P. S. Assay Master of Tin in H. M. Duchy of Cornwall, in a letter to* WILLIAM STURGEON, *Esq. &c.*

Dear Sir,

(a) Two contiguous cells of a glazed earthenware trough were filled with a saturated solution of the *nitrate of copper*,

* It kept the voltaic magnet A. fig. 55, and the rotating star of Barlow, B, working and made a piece of platina wire C red hot, so as to ignite a brimstone match at the same time.

in one of them was placed a piece of vitreous copper ore, and in the other a lump of copper pyrites. They were connected by a copper wire, and after remaining there two or three weeks, *no change was produced on either of them.*

(b) The same steps were pursued with similar ores, for a like time, only substituting the *sulphate of copper* in solution for the nitrate (a). The vitreous ore remained unchanged, but the pyrites became at first *iridescent*, and then *rather purple*,* but the appearance was superficial, and it did not pass into the vitreous.

(c) A similar process was adopted, only replacing the nitrate or sulphate of copper (a, b,) by a solution of the *sulphate of iron*. The vitreous copper ore was unaltered at the end of three weeks, but the *pyrites exhibited a play of colours*; strikingly different, however, from that in the last experiment, its tone being much paler, and the morone tint much less prevalent; in short, it was exactly the hue of *iridescent iron pyrites.*

(d) The experiment being similar to the three foregoing (a, b, c), but in the substitution of a solution of the *sulphate of zinc*, instead of either of those before employed; at the expiration of the usual time, the vitreous ore, as before, was still bright, but an *iridescence* was induced on the *pyrites.* It differed materially from both the others (b, c), but was rather more like the last of them; it certainly, however, had not the chocolate colour, which I should have expected from a deposit of *blende*. M. Becquerel, however, does not appear to have met with much better success.†

I had seen in these experiments but a mere repetition of those published by M. Becquerel in 1834, and which had been communicated to the Academy of Sciences in 1827; I therefore applied to them the explanation which had been given by that illustrious philosopher, viz. that the changes

* The mineral dealers in this county have "time out of mind," been in the habit of converting the copper pyrites into *peacock* and purple copper, by boiling it in a *solution of the sulphate of copper*, in *a copper vessel;* or in the same *solution, with a piece of copper* in a pipkin.

† "*Sulfure de fer et de zinc.* Ces composés sont trés-difficiles à former par les procédés que nous avons décrits, en raison de leur altérabilité au contact de l'air et de l'eau. Nous sommes parvenus, néanmoins, à obtenir le premier, avec l'hypo-sulphite alcalin, en petits cristaux jaunes, d'un éclat brillant, lesquels se sont décomposés rapidement au contact de l'air. Quant au sulfure de zinc, nous n'avons pu l'avoir encore cristallisé." Becquerel, Traité de l'Electricité, Tome iii. 317. Scientific Memoirs, i. 434.

‡ Traité de l'Electricité et du Magnétisme, Tome i. 339.

induced ON the copper pyrites were *deposites or precipitations*, obtaining by THE DECOMPOSITIONS OF THE SOLUTIONS employed. But Mr. Fox has propounded a different solution of the phenomena; attributing it to a DECOMPOSITION OF THE PYRITES, AND NOT OF THE SOLUTION.* Mr. Fox's explanation would require that the pyrites should *diminish in weight*, mine (or rather M. Becquerel's) that it should *increase*.

(e) A piece of copper pyrites from East Pool Mine, and another of vitreous copper ore from Levant Mine, each about a pound and a half in weight, were placed in a glazed earthenware dish, divided by a partition of St. Agnes clay; the two cells being filled with a *saturated* solution of the *sulphate of copper*, and the ores connected by a copper wire. I found the play of colours induced in two or three days, but as it did not appear to increase in depth very rapidly, I put a lump of the *sulphate of copper* on the edge of the dish and in contact with the liquid; it was rapidly dissolved (notwithstanding the solution, when placed in the vessel, was *saturated*,) and the *iridescence increased*, at *length passing into purple*. At the end of seventeen days much sulphate of copper having from time to time been added, the experiment was terminated, the wire connecting the ores having been dissolved by the solution; and the pyrites having been carefully brought to as nearly as could be estimated the same degree of dryness as before the experiment, was found *exactly of the same weight* as it was at first. The liquid from both cells has been examined, and I can perceive *no difference in their colours*, nor is their an appreciable difference in the colour of the precipitates from them with the ferro cyanate of potassa. The whole of the pyrites was not covered by the solution, and the portion of it which projected above the liquid was gradually encrusted with a blue salt, having a slightly greenish tinge; this I find gives a plum-coloured precipitate with the ferrocyanate of potassa, differing certainly from the contents of the cells, but not containing more iron than I think the impurity of the salt employed will account for. The *uncovered* part of the pyrites has become of a deep bronze colour, the line joining it with the purple being sharply defined. The vitreous copper ore was unchanged. The thinness of the pellical of colouring matter on the pyrites, rendering its *weight* inappreciable†; we must have recourse to the other phenomena exhibited in our experiments for deciding the *cause* of the appearances induced.

* Annals of Electricity, i. 134. Athenæum, No. 462. p. 632.
† Nobili. Scientific Memoirs, i. 109.

It has been seen (e) that *sulphate of copper has been abstracted from the solution;* (2) that when the *nitrate of copper* was employed (a), *no change* was induced on the pyrites; (3) that when the *sulphates* of copper (b, e), of iron (c), and of zinc (d) were used, the surface of the copper *pyrites*, exhibited the appearances of the *sulphurets of those metals.*

It thus appears plain to me, that when the *elements of the sulphurets are not present in* the *solution*, *no appearance of them* takes place *on the copper pyrites;* and that when the solutions are *sulphates* (which by *parting with their oxygen only, become sulphurets*) the *sulphurets are obtained.* What becomes of the sulphate which *disappears* from the *solution* if it be not *decomposed*, and its *metallic base and the sulphur of its acid*, DEPOSITED ON the pyrites *in the form of sulphuret?*

Mr. Fox says that "metallic copper in brilliant crystals (mostly octohedrons) was *deposited* on the ore, care having been taken to throw into the cell pieces of the sulphate of copper from time to time as it seemed to be required.* I forbear to speak of the *chemical* objections to Mr. Fox's view of the *change of one ore into others;* but after his (as I think) clear coincidence with M. Becquerel and myself as to the '*modus operandi*' in the case of *metallic copper;* I cannot imagine what has has led him to substitute so much less obvious and easy an explanation of the formation of the *sulphuret.*

Mr. Fox and I do not appear to have obtained the same results, for *in the trough* I obtained no "slightly greenish soluble salt,"† my *solutions being the same in both cells*, and not green in either, the salt to which I have already alluded, was on that part of the pyrites which was untouched by the liquid.

I have but a word or two to offer on Mr. Fox's application of the experiment to the phenomena observed in our mines.

It does not appear to me legitimately deduced that because we can produce crystals of *metallic* copper *on vitreous or purple copper ore*, INVESTING THE PYRITES, *therefore* it explains WHY we in nature find "*metallic copper* frequently *in contact* with grey or black ore, and NOT *in contact with yellow copper ore.*"‡ For in the supposed explanation, the two incompatible substances are *not a line distant from each other.*

* Annals of Electricity i. 134. The Italics are my own.

† London and Edinburgh Journal of Science, x. 171.

‡ Ibid. and Annals of Electricity, i. 134.

Nor do I see here, or in anything yet discovered, WHY black copper ore should be nearer to the surface than copper pyrites.

Mr. Fox lays great stress on the decomposition of the muriate of tin, by voltaic electricity, giving *oxide of tin* at one pole, and *the metal* at the other; but inasmuch as we have *never discovered native (metallic) tin,* I do not think the experiment of any practical application. For notwithstanding in most cases the veins which yield copper ores, do not afford those of tin also, in large quantities; yet there are a very great number (Marazion mines, Levant, Huel, Darlington, &c.) in which the ores of the two metals are so intimately mixed, as almost to defy attempts at separating them; the "dressing" being one of the most difficult, tedious, and expensive of our mining operations.

I have been under many obligations to Mr. Fox, and it is with great regret I find myself differing so entirely from his opinions.

I remain, dear sir, your faithful humble servant

W. J. HENWOOD.

4, Clarence Street, Penzance,
March 11, 1837.

XLIV. *A description of an Electro-magnetic Repeater, or of a Machine by which the connexion between the Voltaic Battery and the helix of an Electro-magnet may be broken and renewed several thousand times in the space of one minute. By the* REV. N. J. CALLAN, *Professor of Natural Philosophy in R. C. College, Maynooth.**

Fig. 52, Plate VII. is a side view of the machine which I have devised for rapidly breaking and renewing communications with the voltaic battery. This machine consists of a mahogany base A B, of the swing wheel V N O (Fig. 53.) of the pallets R S and of the five mercury cups, C, D, E, F, and G. The swing wheel is supported in a vertical position by its axis, which rests on the upright brass plates, *o*, *x*, and *r*, *s*, *t* (Fig. 52.). The teeth of the wheel V N O appear at *n*, between the two brass plates. The wheel is turned by the handle H. The pallets are also supported in a vertical position by the axis M Y, which rests on the upright brass plates, *o*, *x*, and M L. To the axis M Y of the pallet is soldered at K, a thick copper wire *a*, *b*, which is bent so as to form a right angle at each end. At K, under the axis M Y, is soldered a short wire (not shown

* Communicated by the Author.

in the figure) which is always immersed in the mercury of the cup D. The mercury in the cup C, is always connected with the mercury in the cup E, by the bent copper wire *e, e*. To each mercury cup is fitted a cover in which holes are made for admitting wires into the mercury. The beginning of the thick wire coiled round the electro-magnet is immersed in the cup E and the end of the same wire in the cup F. The end of the thin wire soldered to the thick one and coiled on the electro-magnet, is immersed in the cup G. One end of the voltaic battery is always connected by a copper wire with the mercury in the cup D, and the opposite end with the mercury in F. When the extremity *b*, of the wire *a b*, is immersed in the mercury in the cup E, the voltaic current from the battery passes through the thick wire coiled on the electro-magnet, and magnetizes the iron bar. When the same extremity *b* is raised out of the mercury, the communication between the battery and the helix of the magnet is broken, and an electric current capable of giving a shock is excited in the magnetic helix. When the extremity *b* is raised out of the mercury in the cup E, the opposite extremity *a*, of the same wire is depressed and immersed into the mercury in C, and the communication between the magnetic helix and the battery is renewed. Thus by every oscillation of the wire *a b*, the communication between the helix of the electro-magnet and the battery is broken and renewed. If there be thirty teeth in the swing wheel V N O, one revolution of that wheel will cause 60 oscillations of the pallets R and S, and consequently 60 oscillations of the wire *a b*. Hence by turning the handle H, once in a second 60 electric currents of high intensity will be produced in the magnetic helix in the space of a second; or 3600 electric currents in one minute. These currents will pass through any body interposed between the beginning of the thick wire and the end of the thin wire, that is between the mercury in the cup F or C, and the mercury in the cup G.

Maynooth College. N. J. CALLAN.

XLV. *On the communication of Magnetism to Iron by Electrical currents; and on the production of Electrical currents by magnetic induction*, by P. Cooper, Esq.*

There are few scientific subjects which at the time of their discovery excited greater interest, or which have since evaded theoretical generalization more completely, than the communication of magnetism by electrical currents, and the production of electrical currents by magnetic induction.

* Communicated by the Author.

In attempting to give an explanation of these phenomena, I shall be able to make myself better understood within the limits to which this communication will be confined, by referring to the sketch of a theory of natural philosophy which I have recently published;* but I shall at the same time give a concise description of such parts of the theory as apply to the subjects under consideration, that this reference may be dispensed with by those who are not in possession of the abstract.

It is assumed in this theory, that bodies are formed of matter, consisting of globular atoms of different sizes, having an attraction for each other in the direct proportion of their bulk, or quantity of matter, and inversely as the square of their distance; and of light, consisting of globular atoms, constantly separated by a repulsive force, regulated by the same law of distance, uniform in size, and much smaller than the atoms of matter; for which they have an attraction, also regulated, both with regard to distance and dimensions, by the laws already mentioned.†

The light under the influence of these laws must surround the atoms of matter, forming what we have called an atmosphere about each atom; and this atmosphere will be held in its position by various degrees of force, which will draw the atoms of light nearer to each other as they approach the atom of matter, and thus give it greater intensity. The atmosphere, it is supposed, will be divided into strata of different intensities, forming concentric spheres; every atom of light at equal distances from the centre, being held in its position by equal forces.(2).

* The abstract of a series of papers entitled "Outlines of a Theory intended to connect the operations of nature upon general principles." The figures refer to the paragraphs to which corresponding numbers are prefixed.

† Instead of making the division of matter into atoms an assumption to be proved by its subsequent application to the phenomena of nature, we might have taken it as a fact already established; at all events, few persons acquainted with the present state of chemistry will be disposed to object to the admission of this division upon the terms we have proposed it. The proof we have to offer that our assumptions are founded in truth, is, that by admitting them as principles, all the operations of nature, usually included in the physical sciences, may be traced as consequences of the forces which, upon mechanical principles, will necessarily be brought into action. The application of these forces to their different objects is of the most direct kind, and, when properly explained, exceedingly simple; but our present limits preclude us from entering upon the subject in detail.

Under these circumstances, when the atoms of matter are of equal size their atmosphere will preserve an uniform state from the equality of the forces in all directions; but when the atoms are unequal in size, the forces at the points in contact will be unequal, and the positive atoms, or such as have the greatest force, will repel the atmospheres of the negative atoms, or such as have a less force, at the points at which they are in contact, so as to give to these atoms positive and negative surfaces on their opposite sides. (15) The atmospheres of the positive atoms will also acquire positive and negative surfaces; because the light which forms these atmospheres will flow towards the sides in contact with the negative atoms; (6) and as this inequality will give these surfaces an attraction for each other, (7) the atoms, if at liberty to form such an arrangement, will unite in an order alternately positive and negative, connected by alternate positive and negative surfaces. That is, the positive surface of the positive atom will unite with the negative surface of the negative atom; and the positive surface of the negative atom with the negative surface of the positive atom in succession, as in the annexed figure; and there will be no disposition in the atmospheres of these atoms, while thus united, to return to a natural state.*

P N P N P N, &c.

—o+ —o+ —o+ —o+ —o+ —o+

When atoms are united into masses by cohesion, they are still subject to the same laws; the light which forms the atmospheres of these atoms flows towards the surfaces conti-

* When atoms are of uniform size, and are consequently surrounded with uniform atmospheres, although every atom of light repels every other atom of light, there is no derangement produced in these atmospheres by their action upon each other, because the forces being equal in all directions, the repulsion at one point is met with a corresponding resistance at the opposite point. But when atoms of different sizes, and consequently with atmospheres which present different forces, are in contact, the atom which presents the atmosphere with the greatest force, repels the contiguous atmosphere of an atom with less force, until the resistance caused by the accumulation of light upon its opposite surface, becomes equal to the force by which it is repelled to this surface. This arrangement is followed by a consequent series of actions and re-actions between the two atoms, tending to increase the relative forces of the positive and negative surfaces in contact; and when the original difference of force is considerable, the result is an attraction between the two atoms which produces a cohesive union. In this state, the new arrangement, or the derangement, of the atmospheres of the atoms, which form the resulting particle, becomes permanent.

guous to the negative body; so that, whenever two bodies with different electrical forces are in contact, the atoms of both become electrically deranged and present throughout the series alternately positive and negative surfaces. (10)

Now, the electrical fluid, or light, is conducted from point to point by giving the form of a current to the atmospheres of the intermediate atoms; and as the light transmitted has to supply the place of that which is pressed forward, it is required to be of precisely the same intensity; but when the surfaces of the atoms are alternately positive and negative in the line of transmission, this cannot be the case, and we arrive at the remarkable fact that even the metals under these circumstances cease to be conductors. (23).

The remedy which nature has provided for this, has given birth to the science of electro-magnetism. Light has the power to change the direction of the deranged surfaces so as to unite those which are of the same denomination, and consequently of the same intensity, by giving them a polar arrangement in the line of transmission; but as the atoms in this state are subject to the influence of other polar forces, whenever such forces are opposed to this arrangement, whether naturally or otherwise, these surfaces cannot be united so as to form a right line, but assume a spiral direction; the inclination of which to the line of transmission, or to the axis of the spiral, is regulated by the direction and the relative strength of the opposing forces. The galvanometer, by exhibiting the spiral inclination, marks the relative power of the two forces.* (24).

There is another general principle in the theory connected with this enquiry, that whatever may be the character of the derangement impressed upon any atom or section of atoms, it

* The spiral progress of light, thus abruptly introduced, will no doubt appear somewhat hypothetical; but, in the first place, if we adopt the very natural principle of its being pressed forward *like other fluids already in possession of the line of transmission*, of which we have familiar instances in water and gas when conducted in pipes, it is the only arrangement by which light can be transmitted through surfaces which present alternately different intensities; in the next place, it accounts for the motions of the magnetic needle, when presented on different sides of a wire under the influence of a passing electrical current, and other electro-magnetic phenomena, in the most satisfactory manner; and, in the last place, it leads to an explanation of the phenomena of the polarization of light, so complete (the acquired velocity of the current being substituted for a supposed polarity in its atoms, and the spiral turns for Newton's fits) that there is not the slighest difficulty attending its application.

is extended, with all its circumstances, to contiguous atoms in succession, precisely as it exists in the original;* and this extension is made through all bodies indiscriminately, (though they differ in conducting power) from atom to atom until the force is exhausted. This extension of derangement is usually called induction. (14).

We will now endeavour to apply these principles to the objects of our present inquiry.

If we pass an electrical current through a straight wire, free from the influence of magnetic or other polar forces, the positive and negative surfaces of the atoms of the wire resulting from the current will be connected in alternate and parallel right lines in the direction of the length of the wire, and the union of these surfaces will form magnetic circles in the wire at right angles to this direction, so that the current will flow through the wire in right lines.

If we now present one of the poles of a magnet to this wire at right angles to the polar surfaces of its atoms, these surfaces will turn towards the magnet; but as the polar forces of the atoms of the wire are wholly dependant upon the current, which cannot be transmitted except light of uniform intensity be in the line of transmission, the direction of the poles will take an intermediate position, and, instead of being united into circles, as in the absence of the magnetic force, or turned directly towards it in obedience to its influence, which would render the atoms of the wire alternately positive and negative in the direction of its length, and consequently non-conductors, the magnetic circles will be united and drawn into spirals; thus leaving the surfaces of the atoms of the same denomination, and consequently of the same intensity, inclined to the axis of the wires at an angle complimentary to the spiral formed by the magnetic circles; and as these surfaces form the line of transmission, the transmitted light will be continually inclined in passing from atom to atom, in a direction which will give its ray a spiral form, complimentary to the spiral through which they are transmitted.†

* This is finely exemplified in the propagation of sound; which, when properly understood, is one of the most simple and beautiful operations of nature.

† It is impossible in the present state of our knowledge upon this complicated subject, to form a theoretical arrangement with any pretensions to accuracy. It is very evident, however, that if the conducting surfaces be inclined to the line of transmission, the transmitted light must also be inclined, and that it can only recover its rectilineal direction by an equal and opposite inclination in some other part of its course. Now, in any other except a spiral arrange-

The direction of the polar atoms are in all cases regulated by the usual laws of magnetism.

The conducting wire, while under the influence of a passing current, will communicate the derangement of its atoms to the atoms of the air and other surrounding bodies, so as to connect them with itself by alternate, positive, and negative surfaces; and this derangement will be extended from one body to another until the force is exhausted. (14). If then, we coil the conducting wire round a cylindrical bar of soft iron, so as to form it into a helix, the derangement communicated to the iron will be nearly at right angles to the derangement of the wire; that is, the latter having the positive and negative surfaces of its atoms alternately connected

ment of the atoms of the transmitting medium, the inclination would be constantly in the same direction, and, consequently the rectilineal course could not be preserved by a motion backward and forward; but in the spiral, the inclination of every two opposite parts of a coil correct each other. The phenomena of electro-magnetism and polarization, as we have already observed, correspond with these views; (28) the opposite poles of the magnetic, or other polar forces, necessarily producing the opposite arrangement required to give the spirals the different directions which these phenomena indicate. The subject is rendered much more complicated by the consideration that the polarity here spoken of is derived entirely from the transmitted light; so that when we say the poles of the atoms of the conducting wire receive a new direction from magnetic influence, it includes a different connexion of these atoms, regulated by the direction of the current to which the polarity is due.

The disposition in polar surfaces to unite in the manner here spoken of, arises from the attraction of the positive and negative polarities for each other. When atoms of matter are in a natural state, the two forces of attraction and repulsion balance each other: if, for example, we suppose there are 100 atoms of light in action upon each atom at the points in contact, the repulsion of these 100 atoms for each other will neutralize the reciprocal attraction of the same light for the atoms of matter; and as every atom of light repels every other atom of light, and each atom of matter is drawn towards the other by the light in action upon the contiguous surface of the opposite atom respectively, the force of repulsion in this case may be stated at $100 \times 100 = 10{,}000$, and the force of attraction at $100 + 100 = 200$. When, on the other hand, the atoms of matter are brought into a polar state, by the inductive influence of other bodies, by the passing of an electrical current, or by any other means, the atoms of light upon their respective surfaces become unequally divided, and while the attraction continues the same, the repulsion is lessened: if, for instance, we suppose the 200 atoms of

together in circles, the diameters of which are at right angles to the longitudinal direction of the wire, the former will have the positive and negative surfaces of its atoms arranged in alternate planes at right angles to the axis of the bar; so that one end of the bar will terminate with a positive and the other end with a negative surface, and in this state it will have all the properties of a magnet.*

It will be readily perceived, that the unconnected position of the positive and negative ends of this magnetized bar will cause a resistance to the deranging influence of the wire; for, although the surfaces are connected by magnetic curves in the surrounding atmosphere, the air being an inferior conductor of derangement, probably in consequence of its being in this state a non-conductor of light, it will form a very imperfect communication between the ends of the bar. In order then to remove this resistance, we may either bring the two ends of the bar together, so as to form it into a magnetic circle; or, what is more convenient, give to the bar the form of a

light to be divided in the proportion of 120 upon the positive and 80 upon the negative surface, the 120 atoms will be repelled by only 80 atoms, that is $120 \times 80 = 960$, while the two atoms of matter will be drawn together by a force of 120 on one side and 80 on the other$=200$ as before. The balance in favour of attraction will increase with the difference between the two surfaces; and if, to put an extreme case for the purpose of illustration, we suppose the 200 atoms of light to be attached to the positive surface, the whole will attract the opposite atom and render the force of attraction 200, as before; while under these circumstances, the repulsion would be $0 \times 200 = 0$.

See a paper, by the author, on capillary attraction, and on the disposition there is in fluids to assume a globular arrangement. Records of General Science, vol. iv, p. 344.

* This magnetic arrangement corresponds exactly with the theory of Æpinus, as it is corrected in the Treatise on Magnetism, published by the Society for the diffusion of useful knowledge (paragraph 149), I suppose, as it is not otherwise stated, by the author of the treatise. My arrangement was formed, and reduced to writing, several years before I was aware of the existence of this theory, indeed before the date of the only publication in which I have seen it; and I mention the circumstance, not for the purpose of claiming the discovery, the priority of which is of little consequence, but to show that its author and myself have arrived at the same view of the subject by a process of reasoning totally different, and under circumstances that leave no doubt as to the independence of our opinions.

horse shoe, and when we wish to complete the circuit, connect the two ends by means of a piece of soft iron; which, when thus applied, is usually called the lifter.*

It will be observed that the conducting wire, when formed into a helix, will not have its longitudinal direction exactly at right angles to the length of the bar round which it is coiled; but, by a general property of magnetism, (25) when the derangement of the former is communicated to the latter, the superior force of the bar will *fix* the polar surfaces of its atoms in strata at right angles to its axis; and the less powerful atoms of the wire will arrange themselves in obedience to it (26). This will give the magnetic circles of the wire an inclination corresponding with the coils of the helix; and when the wire is required to perform the office of a conductor, these circles will be connected into spirals; and thus provide an uniform medium for its transmission (24).

But although this arrangement will restore the conducting power of the wire, by bringing the positive and negative poles of its atoms into two distinct surfaces, extending spirally throughout the length of the wire, it will render it a non-conductor from one of these surfaces to the other; and if a transfer of light be required from the negative surface of an atom of the wire to its positive surface, or from its positive to its negative surface, it will take the circuitous route of several hundred feet of wire, in preference to the direct course from one side of the atom to the other; and when the ends of the wire are separated by a fluid, it will even reverse the poles of its atoms, in order that the current may pass from one end of the wire and enter at the other end to effect its object (19, 20).

These circumstances appear extraordinary if viewed with relation to a single atom; but when we consider that the atoms of the wire are so connected with each other, that no change in the state of one atom can be effected without a corresponding change in the whole, (10) it will be seen that the plan which nature has adopted is by far the most simple, if not the only means, by which her object could be attained. This object is not to transfer light from the positive to the negative,

* The completing the circuit adds greatly to the power of an electro-magnet, not only by removing the resistance to the deranging force of the wire; but also by the inductive influence of the positive and negative surfaces upon each other when thus brought into communication (16). The great disparity in the lifting and sustaining power of electro-magnets arises from the reduction of the derangement of the magnet when the circuit is broken by the removal of the lifter. (See Phil. Mag. vol. ix. pages 72, 81, & 220).

or from the negative to the positive side of a single atom, but to transfer it from one surface to the other of the whole series of atoms which forms the spiral, so that it may be supplied at one surface simultaneously with its removal from the other; this being the only condition upon which the light attached to any atom can be displaced (11). When, then, the two ends of a wire under the influence of magnetic derangement are in communication, and there is a change in the state of this derangement which requires a transfer of light from one surface of the spiral to the other, the current quits the surface which requires to be reduced at one end of the wire, and enters the surface which requires the addition at the other end; so that at the moment the light leaves one side of any atom, the same quantity is supplied at its opposite side by the other end of the current; the whole spiral surface throughout the wire being in motion, forming an extended and uninterrupted current precisely the length of this surface; every part of which must leave one end of the wire, and enter the other end, before the whole can be brought either into a natural state or to an uniform state of derangement.

Having explained the principle upon which electrical currents are formed, we have now to connect them with the arrangements by which they are usually produced.

We have shown that an electrical current passing through a wire, turned into a helix, round a bar of soft iron, will produce a magnetic arrangement of the atoms of the iron, and give to the bar the properties of a magnet. (14).

If we now suppose the atoms of the iron bar to be fixed under this arrangement, and they will be so if we operate upon steel instead of iron, it will be evident, action and re-action being equal, that if the current were to cease, the wire would still retain its state of derangement, and if removed would recover it upon being replaced. But if this be the case with the conducting wire, it will be the case with every wire under a similar helical arrangement, connected with any part of the magnetic circuit; every part of which, when completed, being in an uniform state of derangement. If, then, we form a helix round the lifter of a bar of iron, in the form of a horse shoe, rendered magnetic by an electrical current, or round the lifter of a similar bar of steel, to which permanent magnetism has been communicated by the same or any other means, upon detaching the lifter from the magnet the atoms of the wire will return to a natural state: to accomplish this, there will be a transfer of light required from the positive to the negative surfaces of the spirals, formed by its previous connexion with the magnet, and this transfer will take place in the manner

we have already described. Upon replacing the lifter a current will pass between the ends of the wire, in an opposite direction, to complete the derangement.

It must be observed that the force which communicates magnetic and electrical derangement, is perfectly independent of the transfer of light necessary to complete it. The disposition to assume the arrangement is in all cases given by the inductive influence of the deranged surfaces which are brought into action, but the light, without which the arrangement cannot be completed, is transmitted, not by the shortest path, but by the route in which it finds the least resistance.—When, for instance, Van Marum charged his electrical battery by means of a galvanic pile, the disposition was communicated by the pile; but the light necessary to complete the derangement was transmitted from the negative to the positive surface of the glass, through the medium of the pile as a conductor; the direct path being obstructed by the electrical character of the glass.

Much of the force of the current produced in wires by magnetic induction, is derived from the accumulated velocity, or momentum, of the current, upon a well known principle common to all fluids;* it is therefore desirable that the current should be established in the wire before its circuit is broken, (to produce chemical or other effects, by the interposition of bodies which resist its progress) but it should be done the instant this is effected, that the current may not be uselessly expended.

Since the preceding part of this paper was written, the views it contains have received further confirmation from an interesting experiment by Mr. Sturgeon; of which particulars are given in the first number of the Annals of Electricity, &c. page 67. In this experiment a conducting wire, coiled in the usual manner, formed the medium of communication between the zinc and copper plates of a voltaic battery, the ends of the wire being at the same time held by the person who was to receive the shock upon breaking the battery communication. The wire under these circumstances would be in precisely the situation of a conducting wire, coiled round an electro-magnet, the arrangement of the atoms of which, as we have already observed, is in every respect the same as in a wire coiled round the lifter attached to the same magnet, but through which there is no current; the removal of the current, therefore, by detaching the wire which completed the communication, would bring the coil into exactly the same situation, as the coil round

* The water ram of Montgolfier is constructed upon this principle.

a lifter when battery communication is broken; and the shock would be given to the person holding the ends of the wire by the reverse current required to bring it to its natural state, upon the principles we have already described.

When the atoms of the conducting wire are arranged in magnetic circles, and the light is transmitted by it in right lines, in the manner we have before stated, upon the cessation of the current, the atoms forming each circle being united by positive and negative surfaces, readily compensate each other by a transfer of light from the former to the latter, and the wire returns to its natural state without any current; but if we conceive these magnetic circles to be joined to each other in spirals, it is evident this mutual compensation cannot take place, because the last link in the circle, instead of being in contact with its former companion, is now joined to another circle, and this to another in succession throughout the wire, leaving the two ends of the spiral at the opposite ends of the wire unconnected.

Now, if the two ends of the wire were united so as to make the junction in every respect uniform with the other part of the wire, and thus render it a homogeneous circle, this mode of compensation might still take place by a *simultaneous* transfer from atom to atom throughout the spiral circuit: but as the slightest interruption, even the interposition of a single section of atoms of a different capacity, would prevent this transfer, the means that presents the least resistance by which the surfaces of these extended spirals can be neutralized, when the spiral circuit is broken, is by a discharge from the positive to the negative surface, in the manner we have already described.

Perhaps this will be better understood from an inspection of fig. 54, Plate VII. Let the atoms *a*, *b*, *c*, &c. united by alternate positive and negative surfaces, form a magnetic circle in the wire through which an electric current is passing; then, upon the cessation of the current, the atoms will return to a natural state by a transfer of light from the positive surface of *a* to the negative surface of *b*; from the positive surface of *b* to the negative surface of *c*; and thus throughout the series, until the transfer is made from the positive surface of *l* to the negative surface of *a*. These transfers must be simultaneous; for it would be impossible to neutralize *a*, by a transfer from its positive surface to *b*, without a corresponding transfer between all the atoms in the series; in such a manner that a quantity of light equal to that which is taken from the positive surface of A, may, at the same instant, be added to its negative surface.

If we suppose that instead of the atoms *a*, *b*, *c*, &c. being connected together in a circle, the positive surface of *a* is inclined to the plane of this circle, so as to connect it with another series of atoms forming the coil of a spiral, and this with others in succession throughout the wire, the negative surface of *a* must be insulated unless the two ends of the wire are brought together, so as to connect it with the positive surface of the terminating atom at the opposite end of the spiral, in the same manner that *a* is connected with *b*, and *b* with *c*, &c.; in which case the transfer may be made simultaneously throughout the magnetic spiral circuit, upon the same principle as in the magnetic circle; but when there is the least disjunction between the points of the wire, and atoms of a different capacity, such as air, water, &c., are interposed, this mode of neutralization cannot take place, because these atoms will not be prepared to receive or transfer the same quantity of light as the atoms of the wire; which, it will be readily perceived, is an indispensable condition in this mode of compensation.

To those who have not duly considered the subject, and who are unacquainted with the connected state of the atoms of bodies under the influence of magnetic or electric derangement, and the simultaneous character of the discharge required to bring them to a natural state, which is the necessary consequence of this connexion; the circuitous mode of compensation, arising from the want of conducting power in the more direct course, here described, will appear, at least, extraordinary; but if they recollect, that when a plate of zinc and a plate of copper, in metallic contact, are plunged into acidulated water, the electric current will take an equally circuitous route through a conducting wire from one plate to the other in preference to the direct course by the surfaces in contact, they will perhaps view it in a different light; and, before they reject it as incredible, be induced to give the subject the consideration which its importance demands.

We do not pledge ourselves with regard to the details of this explanation, which we produce as an approximation to be corrected by further investigation; but the general principles upon which it is founded are supported by their application to such a variety of phenomena, and they so completely connect all the known operations of nature, that we are confident, when the evidence thus widely extended is fully developed, there will not be a doubt of their correctness.

XLVI. *On the production of Insects by Voltaic Electricity. By* Andrew Crosse, Esq., *&c, &c. Communicated in a letter from* Sir Richard Phillips, *to* W. Sturgeon, Esq. *&c. &c.*

Dear Sir,

This morning I received the enclosed from Mr. Crosse, of Broomfield. Its contents are so novel and interesting that I am persuaded you will, even at this eleventh hour, give it place in your forthcoming number. With my best wishes for the success of your valuable Journal,

I am, respectfully,
R. PHILLIPS.

Cadogan Place,
March 25th. 1837.

The following is the account of two experiments in which the birth of Insects was the unexpected result.

Broomfield, March 23d. 1837.

Dear Sir,

Experiment 1. I poured a dilute solution of silicate of potash, super-saturated with muriatic acid, into a quart basin standing on a piece of mahogany in a wedgwood funnel, and caused the fluid to fall by successive drops into the funnel, by means of a strip of flannel wetted with the solution which acted as a syphon, and was suspended over the edge of the basin. These drops were made to fall upon a piece of somewhat porous red oxide of iron from Vesuvius, which was supported by a glass funnel which conveyed the fluid into a bottle below, and which, when full, was emptied back into the basin above, without disturbing the position of the stone. Two platina wires at the opposite ends of the stone, were connected with the positive and negative poles of a voltaic battery, consisting of nineteen pairs of five-inch plates in cells filled with water and $\frac{1}{500}$ muriatic acid. Thus the stone was kept constantly electrified by the battery, and moist by the dropping. At the end of fourteen days, two or three very minute white specks or nipples were visible on the surface of the stone, between the two wires, by means of a lens. On the eighteenth day these nipples elongated and were covered with fine filaments. On the twenty-second day their size and elongation increased, and on the twenty-sixth day each figure assumed the form of a perfect insect, standing on a few bristles which formed its tail. On the twenty-eighth day these insects moved their legs, and in the course of a few days more, detached themselves from the stone, and moved over its surface

at pleasure, although in general they appeared averse to motion, more particularly when first born. In the course of a few weeks more, about a hundred of these insects made their appearance on the stone. I observed that each of them fixed itself for a considerable time in one place, as far as I could judge appearing to feed by suction; but when a ray of light from the sun was let fall upon it, it seemed much disturbed, and finally removed itself to the shaded side of the stone. I ought to state that out of about a hundred insects, not above six or seven were born on the south side of the stone. I examined some of these insects with the microscope. Their shape was similar to that of the cheese mite, but they seemed to be from twice to eight times the size of that animalcule, and the motion of their legs was occasionally discernible by the naked eye. They had in general eight legs, but there were some with only six; their bodies were covered with bristles which gave some of them the appearance of a little star; the length of the bristles at the tail was considerable, and these when highly magnified appeared spicated. Instant death followed their removal from the stone however delicately performed, but in the course of time most of them were washed down into the bottle below, and poured into the basin above, from whence they found their way to the wooden support which was kept moist by the fluid dropping between it and the wedgwood funnel. On examining this piece of wood I found upwards of forty living insects actively crawling over its surface, and apparently feeding on the silicious matter with which it was impregnated.

Experiment 2. I filled a small glass jar with a concentrated solution of silicate of potash, and plunged into it a thick iron wire connected with the positive pole of a battery composed of twenty pairs of cylinders in cells filled with common water, and immersed in the same a small coil of fine silver wire connected with the negative pole of the same battery. In the course of a few weeks gelatinous silex was formed in some quantity round the iron wire, and after a longer interval of time, a similar formation, but in much less quantity, filled up the silver coil. On examining these wires with a lens, I first observed a similar insect at the negative wire, and some time afterwards I found two more; in the whole, three at the negative, and shortly afterwards I found twelve at the positive; in the whole fifteen. Each of these insects was well formed, and quite as large as those observed in the first experiment. They were deeply imbedded in the gelatinous silex, with the bristles of the tail projecting. They were in general from half to three quarters of an inch below the surface of the fluid. In

this experiment there was neither flannel, wood, volcanic stone, nor acid, consequently the insects appear to have been born in the silex. Not being an entomologist, I can give no opinion on the matter. I have only related facts. Many persons have seen the insects both dead and living. Whether the electric action has any thing to do with their birth or not I cannot say without further experimenting. Perhaps if these experiments were repeated and varied, curious facts might be the result. Since the above, having found that all the insects in the first experiment had died, I poured into the basin an additional quantity of the same fluid, and in the course of two or three weeks I had a second crop of insects upon the volcanic stone. I have also had two other formations in two distinct solutions of silicate of potash; also five others in the following solutions: 1, a weak solution of oxide of copper; 2, 3, 4, 5, in *concentrated* solutions of green sulphate of iron, sulphate of zinc, sulphate of copper, and *nitrate of copper*—in the whole, *ten* separate formations; *five* in silicious solutions, and five in solutions of metallic salts &c.—each of them was exposed to a long continued electric action, before any insect made its appearance, still without further experimenting I would not venture an opinion. I hope in the course of time to know something more about this. I am going on well in crystallization. I have had a very handsome letter from Mons. Becqueril.

Your's sincerely,

(To Sir Richard Phillips.) ANDREW CROSSE.

XLVII. REVIEWS & NOTICES OF NEW BOOKS.

MICROGRAPHIA; *containing Practical Essays on Reflecting, Solar, and Oxy-hydrogen gas Microscopes; Micrometers, Eye-pieces, &c. &c. By* C. R. GORING, *M. D.; and* ANDREW PRITCHARD, ESQ., *M. R. I. Hon. Mem. Soc. Arts, Edin., &c.* WHITTAKER & CO.

This is one of those rare books in which nothing is left wanting to fulfil to the uttermost the pretentions of its title. Its essays are not only practical, but are, without exception, the very best practical essays we have yet met with on these interesting subjects; and are written in that clear and unsophisticated style which characterizes every Author who is perfectly master of his subject, and who is capable also of clothing his ideas in that language by which the rich stores of information, which from a well directed study, and habitual observation, he has accumulated, may be the most extensively and usefully disseminated.

No one who has perused the MICROGRAPHIA can hesitate to recommend it to all those who are desirous of becoming acquainted with the structure, management, and powers of the most approved microscopes of the present day. Every essay in this book conveys to the reader a fund of useful information, of which, both veteran and less experienced observers, would do well to avail themselves. To the latter class of observers this book cannot be too strongly enforced upon their notice, as it supplies a desideratum they have long experienced, by furnishing them a means of descrimination in their observations, and in the selection of microscopes, which in no other book is to be found.

Contents of the MICROGRAPHIA. History and description of Reflecting Microscopes—on Micrometers, and their use—Monochromatic Illumination—Solar Engiscopes, and the exhibition of tests by them—on trying Microscopes and Engiscopes against each other, with rules for ascertaining their comparative merits—on Eye-pieces—Illustrations of Angular aperture—on the most improved construction and management of Solar and Gas Microscopes—Cuvier's method of dissecting objects in fluids—on a new and simple fine adjustment for Microscopes—on making drawings of inanimate Microscopic objects, by F. Bauer, Esq. F. R. S. &c.—on a new method of illuminating objects, by the Rev. J. B. Reade, M.A.

The Micrographia is illustrated by splendid copper-plate engravings, and numerous elegantly executed wood cuts.

XLVIII. MISCELLANEOUS ARTICLES.

A letter to W. STURGEON, ESQ., *&c.*

Leicester, Dec. 22d. 1836.

My Dear Sir,

I have read the first number of your "Annals," and mean to take it in regularly, so long as it continues to support its character. It is the only work that I have ever seen which is quite to my mind: a work which was wanted long ago in the scientific world, and one which, I am quite sure, will be eagerly received by all who, like myself, are anxious to obtain direct information on the subjects it professes to embrace; without, at the same time, paying for an Encyclopædia, or a mass of other matters, how important soever those matters may be in themselves.

I hope I shall never undervalue any one branch of literature or science; but I do think it a hardship that one cannot purchase a paper on such a simple subject, for instance, as the most efficient method of firing gunpowder by the electric spark, without paying for abstruse essays on mathematical subjects, and perhaps for some lengthy papers on the subject of the circulation of the blood, or on geology, or it may be phrenology.

There seems to me to be no necessity now, whatever there might have been in former times, for giving our scientific periodicals that peculiar *caste* which they all assume. They start with a promise of endless variety, and, in a few short months, settle down in dull monotony, their pages exhibiting little else than the symbols which are to be found in the front of any of our mathematical dictionaries.

The path you have marked out for yourself is one strewed with the very exuberance of those productions on which the development of all that is important to the present unprecedented progress of scientific discovery depends. Walk right on in that path; deviate from it neither to the right hand nor to the left, and your Journal will soon rise to a degree of eminence that your most sanguine expectations could scarcely have anticipated.

I am not writing, my dear sir, as a candidate for the honour of appearing in your pages; my chief motive is to congratulate you on the appearance of your work, and my next is to suggest to you a topic on which, I think, a paper from you, or from some of your able correspondents, would be truly interesting. The subject to which I allude is the imitation of falling stars by common electricity. I have tried this experiment with an instrument made and sold for the purpose, but without effect. But I must do justice to the subject. The instrument then, was a glass tube about 4 feet long, capped at both ends, one cap terminating within the tube in a ball, the other in a point: the diameter of the tube about an inch and a half. Jars of all sizes I tried to discharge through this tube: the tube, let it be remarked, was tried in various degrees of exhaustion, but no effect was produced. Finally, it was tried repeatedly, with two large jars containing about twelve square feet of coated surface, but no falling star appeared. As an instrument for showing the aurora borealis it acted beautifully, but for the purpose for which is was made it was of no more use than a piece of deal board. Singer, in his elements of electricity, tells us that he used a tube "five feet in length, and 5-8ths of an inch diameter, capped with brass at each extremity." It would have been well had he said how these caps terminated

interiorly, and if he had any peculiar method of passing through such tube the discharge of the very large jar which he says he employed. See p. 264 of his work. The absence of this information I consider as a great defect in the work; and, by the way, is is a defect most prominent in many works of this description.

Now, my dear sir, might I ask the favour of your treating this subject in your Annals, at your leisure? If the experiment be such as Singer describes it, it must be a very fine one, but sure I am that no one from his imperfect account can repeat it. The charge that I employed will readily fuse eighteen inches of pendulum wire; and I am willing to believe that there may have been some oversight in my method of effecting the imitation of the falling star, but I do assure you the circuit was made complete; yet the strong charge only hissed.

You are quite at liberty to make any use of this letter you may think proper; all I wish is such definition as will ensure success, and I venture to say that such definition has not yet been given. When I put the instrument to the test, it was with the assistance of the person who furnished me with it.

With the most hearty wishes for the success of your excellent work, and in the hope that, late in the month as it is, you will favour me with an acknowledgment in your notice to correspondents, in any way you think proper.

I am, my dear Sir,
Your's, very sincerely,
JAMES MITCHELL.

Answer to Mr. Mitchell's Letter.

My Dear Sir,

I am extremely obliged by the polite manner in which you have been pleased to express the sentiments you entertain of the periodical I have ventured to present to the notice of philosophers; and can assure you, whilst in my hands, nothing but what is instructive, in the subject it professes to promulgate shall enter its pages. The "Annals" may possibly have hitherto appeared deficient in foreign intelligence, but that deficiency has been occasioned by the arrival of communications from our home correspondents, to a greater amount than was anticipated; and has now accumulated to an extent that will require the publication of an extra number to bring up the arrears: which number will appear on the first of May. After the present volume is completed, I hope to be enabled to make proper and satisfactory arrangements for the

regular introduction of every thing, foreign and domestic, that is worthy a place in the "Annals."

With reference to the *falling star* experiment, I am not certain that I can give you much better information than you have found in Singer. That author gives the best instructions I ever met with; and, as far as my experience is concerned, they are perfectly correct. If any part be omitted it is the metallic termination in the tube, which ought to be a very small well-polished ball. There is, however, a sad mistake in fitting up several kinds of electrical apparatus, which I think is likely to attend your tube. I mean the terminating of tubes and other glasses with *sharp-edged* cylindrical brass caps, which, in all cases, throw off much fluid into the surrounding atmosphere. With *shouldered* glasses, these caps are not so injurious; but cylindrical glasses, as tubes, &c. ought always to be capped with spheres of sufficient size to admit the tube by an opening in one side whose diameter does not exceed a chord of 90°. The less the chord the better; and the edges of the hole to be rounded and well polished. A wooden or ivory cap is better than a metallic one. It may then be cylindrical, with rounded edges; and a brass wire, with the necessary terminations, may pass through its end.

Your charged surface was more than sufficient to exhibit the falling star. More depends upon the intensity of the charge than upon the extent of coated surface. The air within the tube ought not to be too much attenuated; and the tube itself not too wide; half an inch diameter is quite wide enough to ensure success when the tube is more than three feet long. When the column of attenuated air is not more than twelve inches high, its diameter may be four inches. The fluid from two square feet of inside coated surface, and I suppose less will do, intensely charged, makes a fine falling star when transmitted through attenuated air in a tall *mercurial shower* receiver, standing on the pump-plate; its upper end being covered with a flat plate. If a ball be stuck by cement, or screwed to the under-side of the top plate, it will succeed better. When the receiver is wide it is difficult to succeed. You will now see that your tube is too wide; and I should advise that there be no point inside any other you may fit up for this experiment. A small ball should be inside the upper end, and in the lower end either a ball or a flat surface. To this latter you may employ either a valve or a stop-cock.

I am, Dear Sir,
Your's truly,
W. STURGEON.

For the information of other readers.

"If electricity be passed through an exhausted receiver gradually, it assumes the appearance of the Northern Lights; but if a considerable electrical accumulation be suddenly transmitted, it will pass through the receiver with all the straightness and brilliancy of a falling star. If the receiver is six inches diameter, and fourteen or sixteen inches high, the full charge of a moderate sized jar is necessary to produce this effect, and it occurs most readily when the receiver is but moderately exhausted, so that the rarefied air it contains may have some degree of resistance.

"The artificial imitations of the two phenomena, therefore, require the same conditions for their production as appear to obtain in nature; for the aurora occurs in the highest parts of the atmosphere where the air is most rarefied; and the most accurate imitation of its appearance is obtained in the most perfectly exhausted receiver: falling stars take place much lower, where the air has more density; and to imitate them, it is necessary to employ a medium that opposes some resistance.

"These facts are confirmed by almost every possible variation of the experiment, and in some instances the approach to the appearance of the natural phenomena is remarkably striking: for the electric fluid may be made to pass over a very considerable interval by the employment of a proper apparatus.

"I employ for this purpose a glass tube, five feet in length and 5-8ths of an inch diameter, capped with brass at each extremity. When this tube is exhausted, no ordinary spark will pass through it in any other than a diffused state; but by employing the charge of a very large jar a brilliant spark is obtained through the whole length of the tube. Mr. Morgan found that in a shorter tube of the preceding description, the appearance of a falling star was produced by a spark which would pass through ten inches in the open air, provided the tube did not exceed forty-eight inches in length, and contain a quantity of air, which under common circumstances would have filled one twenty-fourth of its capacity; but if this small quantity of air was further dilated by the action of an air pump, the most powerful spark would pass through it in a divided stream. By employing a very narrow tube of the same length, the confined column of rarefied air resisted the charge sufficiently to produce the appearance of

the brilliant spark through its whole length, whenever the accumulated electricity was sufficiently powerful to pass through it." *Singer's Electricity.*

We have just been informed that a new and definite compound of carbon and hydrogen has lately been discovered by Mr. Maugham, Lecturer on Chemistry, at the Adelaide Gallery. This body is said to consist of one atom of carbon and one atom of hydrogen, consequently its atomic weight is seven. It differs entirely from the other compounds of these bodies; and, in our next number, we trust to be able to give an account of its properties and the mode of preparing it. Mr. Maugham, we understand proposes to call it *protohydruret* or *protohydroguret* of carbon. EDIT.

In the first number of these "Annals" I have described an Electro-magnetic Engine, by means of which, pieces of machinery are put in motion. I have now to announce that I have succeeded in propelling a boat, and also a loco-motive carriage, by the power of Electro-magnetism. The particulars of their construction will be communicated as soon as their present rude state is sufficiently corrected for their appearance in public.

W. S.

THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

MAY, 1837.

XLIX. *On the theory of Magnetic Electricity.* *By* WILLIAM STURGEON, *Lecturer on Experimental Philosophy, at the Honourable East India Company's Military Seminary, Addiscombe, &c. &c.*

(Continued from page 200).

I believe it is generally admitted by writers on magnetism, that a steel bar in a state of polarization is surrounded on every side by the magnetic matter, frequently called the *magnetic effluvium,* which forms to the bar a species of magnetic atmosphere. This point being granted, it will be a matter of no consequence to the present undertaking, whether this effluvial matter be stationary as regards the magnet, or whether, as some have imagined, it be continually flowing from pole to pole:- it will be sufficient for the present purpose to consider it as consisting of exceedingly minute polarized particles, emanating immediately from the surface of the steel; —concessions of no novel character,—and such, I imagine, as but few will be found willing to deny.

With regard to the *distribution* of the virtual intangible magnetic particles in the vicinity of the bar, we cannot perhaps be more correctly directed for information than by examining attentively the arrangement of fine particles of iron, when gently and promiscuously scattered on paper, beneath which is placed a magnetic bar; for, notwithstanding the magnetic matter itself,—in consequence, perhaps, of the exceeding minuteness of its particles,—escapes the cognizance of vision, the distribution of the ferruginous particles being accomplished by its polarizing efficacy, may very justly be considered as the true representative of the distribution of the virtual intangible magnetic matter enveloping the surface of the steel.

Now, as those elemental magnetic intangibles are polar, their poles will necessarily be arranged according to the immutable laws exhibited by visible tangible magnets; to which

they are the main-spring of all their energies, and the only active agents by which their mysterious phenomena are called forth, as displayed in the silent motions of the passive obedient steel. Regular concatenations of alternate *north* and *south* poles, will, by their mutual attraction, pervade every part of the magnetic effluvium as decidedly and as uniformly as in a consecutive series of polarized ferruginous bars.

Under these considerations it will readily appear, that all the elemental magnetic particles enveloping the *north* portion of a regularly magnetized bar of steel will have their *south* poles directed towards the surface of the metal, and consequently all their *north* poles will be directed outwards in every part of the arrangement. Precisely the reverse of this distribution of poles will take place in the magnetic matter enveloping the south portion of the steel; so that in this case the north poles will be directed towards the south portion of the metal, and consequently all the south poles will be turned outwards.

If now we contemplate the arrangement which would take place in the vicinity of one polar portion only, of a piece of steel, supposing it to be uninfluenced by a pole of the other kind, we shall discover, by the laws of magnetics, that the polar affections of the enveloping magnetic matter will arrange the particles of which it is composed, into radial polar lines, emanating from every part of the steel surface; for, as each individual line will be formed by the attachment of a consecutive series of dissimilar poles of elementary particles, the remote extremities of all these virtual magnetic lines will become similarly polarized: in consequence of which, they will have a constant tendency to diverge from each other. Hence if we be contemplating the north polar portion of the steel, the remote extremities of the virtual magnetic lines will be north polar; but if it be the south portion of the steel which comes under consideration, the remote extremities of the magnetic lines will be south polar. Hence also, the lines of magnetic action which envelope a bar of steel displaying two poles only, may be divided into two distinct classes or systems; one of which may be called north polar, and the other south polar. If it were possible that either of these systems of magnetic lines could be displayed separately and independently of the disturbing force of the other system, those lines would be perfectly straight, or without flexure in every part of their course; that is, they are naturally right lines; and if the magnetized body were a sphere, the virtual polar magnetic lines would radiate in right lines from every part of its surface. (See fig. 59, and 60, Plate IX.)

Hitherto I have endeavoured to explain what I consider radiating magnetic polar lines, emanating without obstruction, from a magnetized piece of steel, under the supposition of its being unipolar on every part of its surface: but as no piece of steel, of whatever form it may be made, has yet been known to exhibit one uniform polar state, but on the contrary, each piece of magnetized steel invariably displays a plurality of poles, and one at least of each description,—it will next be necessary to take into consideration in what manner the two systems of polar matter affect each other, and in what manner the elementary polar lines of each system become deflected out of their natural rectilinear course by their mutual attraction of each other.

If fine steel or iron filings be gently scattered on a sheet of card paper, under which is placed a bar magnet, they will immediately become polarized by the influence of the magnetic matter enveloping the bar; and if they be slightly agitated by hitting the paper a few gentle taps with a pencil or other such light body, they will become arranged in multitudes of exceedingly fine lines, some of which will be straight, and others curved, as in fig. 61. Conspicuous lines, each with a dash across one of its extremities, are drawn, to show their general positions in each system.

In this arrangement of the ferruginous particles we have perhaps a pretty correct picture of a longitudinal section of the distribution and arrangement of the intangible magnetic matter enveloping the steel bar. Near to, and around the extremities of the bar, the two systems of polar lines proceed nearly in their natural rectilinear direction; but those polar lines of each system which are more vicinal to the neutral point, or to the neutral line *e q*, which crosses the centre of the bar, in consequence of presenting poles of different characters outwards, do, by their mutual attractions, aberrate from their natural course, and bend or incline towards each other; forming curves of different degrees of flexure, according to the powers of their reciprocal forces, and their distances from each other. If the steel bar be cylindrical, and uniformly magnetised on every side, then, whatever longitudinal line of this magnet be turned upwards, or towards the paper, a similar arrangement of polar lines will be exhibited, demonstrating in the most satisfactory manner that the virtual polarizing magnetic matter completely envelops the ferruginous cylinder. Fig. 59 & 60 represent the distribution of fine particles of iron when strewed on paper above the ends of the cylindrical magnet.

Fig. 62 is a representation of the arrangement of fine particles of iron, strewed on paper above a horse-shoe magnet,

which affords a tolerably exact idea of the direction of the invisible polar magnetic lines as they are distributed in the plane of the magnet. Fig. 63 represents the arrangement and distribution of iron filings, scattered over a transverse section, or over the poles of the same magnet.

In fig. 62 it is observable that the magnetic polar lines exhibit the greatest degree of aberration from their natural rectilinear direction, in front of the metallic poles, whilst but very trifling deflections of the polar lines are to be seen, along the edges of the magnet; even on the outside of the limbs the aberrations are much less than those exhibited on the surface of the bar magnet fig. 61 at similar distances from the poles.

In this case the magnetic polar lines maintain their natural rectilinear direction, even at considerable distances from the extremities of the metal, and particularly between the limbs of the magnet; in consequence of the two systems emanating from the metallic surface in diametrically opposite directions, and meeting each other, as it were in the same rectilinear path. On the outside of the limbs, the aggregate of the two systems of polar lines, in the plane of the magnet, are not only so far separated from each other as to be little affected by their mutual attraction, but are also so situated with regard to the transverse curvilinear forces (see fig. 63), that they form a series of resultant right lines in the plane of the magnet. These lines have, however, a small degree of flexure from their natural course, arising from their mutual attractions in the direction of the metal, which bend them a little towards the centre of the magnet.

Having thus illustrated what I consider to be the virtual polar magnetic lines, and also their most usual arrangements in the vicinity of steel, or other ferruginous magnets, I now propose to show that the excitation of magnetic electricity, and also the direction of the currents excited, are referrible to the agency and position of these polar magnetic lines alone; without any regard whatever to the poles, figure, or position of the steel which they envelop; any further than as those lines are casually arranged on its surface, by the diverse arbitrary forms and proportions it is so frequently made to assume.

The theory of electric excitation by magnetic agency will be embraced in the following positions:—

Position 1.—Magnetic electricity may be excited in all the metals, and perhaps in some other conductors of electricity.

Position 2.—The excitation depends upon a disturbance of the equilibrium of the electric fluid natural to the metal; by its impinging on the exciting polar magnetic lines; and is accomplished by mechanical motion, either of the metallic

body to be excited, or of the magnet,—or of both at the same time. For simplification, however, we will suppose the magnet to be stationary, and the metallic body alone to be put into motion.

Remark.—As the electric fluid by this process has not as yet been recognised in any other state than that of motion, the phenomena are necessarily displayed upon the principles of electro-dynamics. Hence the term "*excitation*" in this place is to be considered not only expressive of a process for simply disturbing the electric fluid, but as one which is capable of communicating to various quantities of it an infinite variety of velocities. And as the quantity of fluid in motion, and the velocity with which it moves, will conjointly constitute an electro-momentum, which at all times will be proportional to the product of its constituent elements; it is therefore the production of the electro-momentum which is to be understood when we speak of various degrees of excitation.

Indeed, whatever may be the nature of the exciting agent, or the mode of its application, it is in this sense only that the term excitation can, with any degree of propriety, be applied when electric currents and their effects are the phenomena under contemplation. Electro-momentum is an expression which at once conveys to the mind the author's meaning,—that it is the production of the velocity multiplied into the quantity of electric matter, which, by the process, whatever may be its character, is impelled into motion from its previous statical repose.

Electric currents generated by a voltaic battery are constituted of distinct alternate charges and discharges of the electric matter, or of electro-pulsations; and may be assimilated to the currents of blood through the animal system, which are produced by the alternate charges and discharges at the heart. And it is very far from being improbable that both are actuated upon the same principle. The electric fluid called forth by a voltaic battery is, therefore, alternately accumulating and discharging during the whole time the instrument is in action. In the former case the intensity is exalting; but it is in the latter alone that the force is exhibited: which force is the production of the quantity of fluid discharged, and the velocity with which it moves conjointly; which may very clearly be understood by the term electro-momentum.

As however the electro-pulsations in most cases are produced too rapidly to be separately considered, it is the aggregate of the multitudinous electro-pulsations constituting the general discharge in a given time, that is to be understood by the term electro-momentum when a voltaic battery is the instrument employed for generating the electric currents.

Thermo-electric currents are also, in some cases, of a pulsatory character; for, as several of the metals are constituted of crystals, and those crystals of distinct elementary metallic films (see my paper on the *Thermo-magnetism* of simple metals, Phil. Mag. and Annals, vol. x.*), the heat, which in this case is the impelling agent, must necessarily arrive at a certain degree of concentration, or of intensity, if you please, in one film, or distinct metallic element, before it can possibly take possession of the next. Consequently, however small and inappreciable may be the interruption in each stage of its progress, each interruption must necessarily produce a virtual pause; the very existence of which in the advances of heat from film to film will constitute a pulsatory progression.

In the Marechausian† *colonne pendule*, or dry electrical column, the electro-pulsations are, in consequence of the very great number of interrupting papers, less frequent than in either the process of Volta, or in that of Seebeck. Notwithstanding which, the instrument produces slow pulsatory currents.

The favourite term intensity, so frequently pressed into the service of some writers, appears to have no definite meaning in the vague manner in which it is generally employed. It occurs, *sine discriminatione*, in electro-statics and electro-dynamics as if no real difference existed in the two distinct conditions of the electric matter; and as it is very far from being expressive of either of them, it can never be intelligibly employed in that double capacity.

When first introduced as a technical term in electricity, it appears that intensity was intended to express degree of an electro-statical charge; and it has never yet been employed to denote distinctly an electric force constituted of quantity and velocity. Intensity, therefore, cannot be considered as synonymous with momentum, which admits not of being warped into electro-statics, nor of being dispensed with in electro-dynamics.

The term induction is in precisely the same predicament as that of intensity, and may very justly be considered as a fellow slave, variously, and often unintelligibly, employed.

Position 3.—When the metallic body moves in any given direction with regard to the polar magnetic lines, the more rapid the motion the greater will be the degree of excitation, or electro-momentum produced; and vice versa, the slower the motion the less will be the degree of excitation. Consequently, when the velocity is at a minimum or nothing, no excitation whatever can exist.

* This paper will shortly appear in the Annals of Electricity, &c.

† M. Marechaux appears to have constructed the first dry electric column.—*Ann. de Chim.* for January, 1806.

Illustration.—If the excited body be of such dimensions as to have the whole of its natural electric fluid put into motion by the process, the electro-momentum would always be proportional to the velocity, because of the quantity of fluid in motion being constantly the same; and as by position 2, the motion of the fluid depends upon the motion of the excited metal, the velocity of the former will at all times depend upon that of the latter; and consequently the electro-momentum or extent of excitation will be proportional to the velocity of the moving body under excitation.

There may possibly, however, be a limit to the extent of excitation by an increase of motion, when the velocity is very great, in consequence of the yielding of the exciting magnetic lines to the force of the moving body, or to its electric fluid whilst striking them with great rapidity. But as far as my experiments and observations have been conducted, I am led to believe that the electro-momentum may be exalted by an increase of motion until the velocity becomes exceedingly great.

Position 4.—When the velocity of the moving body, and the energy of the exciting polar magnetic lines are constant, the maximum of excitation will be accomplished by the body moving at *right angles* to those lines against which it impinges.

Position 5.—When the direction in which the body moves is inclined to the axis of the exciting polar magnetic lines at any other angle than 90°, it receives no more excitation than what is due to the quantity of its motion taken in the direction perpendicular to that axis.

Illustration.—As the excitation of the body, or of the electric fluid which it contains, depends upon its collision with the polar magnetic lines; the greater the number of those lines against which the body strikes in a given time, the greater will be the number of exciting impressions accomplished in that time.

Let $a\,b$ and $a\,c$ (fig. 64), be two directions in which a piece of metal is caused to move: the former perpendicular, and the latter oblique to the axis of the group of polar magnetic lines, represented by the vertical lines dashed across their heads in the figure. If now $a\,b = a\,c$ represent the velocity in each direction, then those two lines will also represent the spaces through which the body moves in two equal portions of time. Now it is evident, by mere inspection of the figure, that whilst the body moves from a to b, in the direction perpendicular to the axis or general direction of the polar magnetic lines, it will have to impinge against a greater number of those ex-

citing lines than whilst moving in the oblique direction from a to c. Or the body will impinge on no greater a number of polar magnetic lines whilst passing obliquely from a to c, with the velocity $a\,c$, than it would strike by moving with the less velocity $a\,d = f\,e$, the quantity of its motion taken in the perpendicular direction $a\,b$.

But, as the velocity is supposed to be constant in both directions, then the same number of exciting impressions will be accomplished by the body being in motion during a part $a\,d$, only, of the time $a\,b$, in the perpendicular direction $a\,b$, as will be accomplished by its being kept in motion the whole of the time $a\,c = a\,b$, in the oblique direction $a\,c$.

Corollary.—Hence it is evident, that if a metallic body were to move in the direction of the axis of a group of parallel polar magnetic lines, it would suffer no excitation whatever. The position is also conformable to experiment.

Position 6.—The natural or primitive channel of an electric current generated by magnetic agency is at right angles to the axis of the exciting polar magnetic lines, whatever may be the direction in which the excited body moves.

Remarks.—The current may, however, be led or conducted in various other directions, according to the figure and dimensions of the metal employed, and the various directions in which it may be put into motion; notwithstanding which, the primitive channel of the current will be constantly the same, at right angles to the axis of the exciting polar magnetic lines.

Position 7.—The *direction* in which the current flows with regard to the exciting polar magnetic lines, is constantly the same, whatever may be the direction in which the metal is put into motion, or to whatever extremity or other part of a magnet the metal may be applied.

Illustration.—Let a, b, c, d fig. 65, be a ring of metallic wire, placed with its plane horizontal, and embracing a bundle or group of polar magnetic lines, the axis of which passes through the centre of, and at right angles to, the plane of the ring.

Let those magnetic lines emanate from the south magnetic pole of a bar of steel placed beneath the paper on which the figure is drawn. Consequently, their south poles (marked poles) will be upwards, and may very conveniently be represented by the group of small crosses embraced by the ring. Fig. 66 is an oblique view of fig. 65.

If now the ring be put into motion in its own plane, it will be a matter of no consequence which side advances towards the centre; the electric current thus excited will flow in every

part of the ring in one and the same direction; which direction is indicated by the four exterior arrows, fig. 65.

Now, as the group of polar magnetic lines is stationary, and encompassed by the ring, it will be that part only of the ring, which *advances* towards the centre or axis of the group, which will receive the exciting impressions. The opposite side, instead of impinging on the polar magnetic lines, absolutely recedes from them, and operates in no other capacity than that of conductor to the excited current in the advancing side. So that whether it be *a, b, c,* or *d,* which advances towards the centre, their opposite sides *c, d, a,* or *b* will respectively recede from the axis of the group, and become conducting parts of the ring, whilst the former correlative parts are receiving the exciting impressions.

Fig. 67 represents the ring cut open in four places, and each part made perfectly straight to represent four separate pieces of wire.

Let any one of these wires advance towards the centre of the group of polar magnetic lines. Then as the excitation in this case is under precisely the same circumstances as in the former, the electric current in the advancing wire, or part of the ring, is also constant and uniform in its primitive direction, flowing in one and the same invariable course, relatively to the exciting polar magnetic lines which gave it birth and activity. (See the arrows in fig. 67).

To familiarize still further this beautiful law of magnetic electricity:—Let any man suppose himself to be placed in the axis of a group of polar magnetic lines, similarly situated to those in fig. 65 and 66. Let him now stand or suppose himself to be standing in the centre of a hoop or ring of metal.

Whilst in this position, let him permit the ring to move in its own plane. Consequently, some part of it will advance towards him, whilst the opposite part will recede from him. The former will receive the exciting impressions, and the latter will become a portion of the conducting circuit.

Let him now look to whatever side of the ring he pleases, the current before him will be flowing from his right to his left hand.

If it be the excitation of a straight wire which he is contemplating, let him consider it as a portion of the original ring, or as one of the straight pieces in fig. 67, permitting it to advance towards his front; his left hand will be the unerring index to point out the direction of the passing electric current.

A walking-stick, or any other such article, may very well represent the metal to be excited; then a person standing in

the position of the polar magnetic lines, as represented in fig. 65, 66, and 67, and holding the stick before him, by its extremities, one in each hand, and at right angles to the axis of his person, or to a straight line drawn from his head to his feet, will, by pulling the stick towards him, show the proper direction of motion for effecting the greatest degree of excitation under the conditions laid down in Position 4; and by the illustration of Position 7, the current would flow through the stick from the right to the left hand.

The preceding positions will, if I have not deceived myself, exhibit a correct view of one class at least of the natural elements of magnetic electricity; viz. those proximate theoretical laws which govern its excitation, and give direction to its polarizing streams. They are those laws by which the display of the phenomena is accomplished and regulated, and by which it may very simply be explained, and easily understood. By these laws the experimenter may be directed in his manipulation, and with precision he may foretel the direction of the resulting electric streams.

It is probable, however, that other laws are in operation during this novel process of excitation, which are still more remotely situated from observation; and require for their development, experiments and a mode of reasoning of a very different order to those which have been employed for organizing the system of laws already explained.

It appears to me that electric currents generated by magnetic excitation perform an active part in bringing into play another agent, whose influence modifies the degree of electric energy to a considerable extent. The polarization of the magnetism of the conducting metal, or of the surrounding medium, or of both, is called into immediate action; and becomes arrayed in polar lines as precisely and as uniformly as those of the ferreous body, which are the first parents or main spring of the whole phenomena. These systems of *true* electro-magnetic lines, operate by laws similar to those which attend the action of the polar lines arranged by direct magnetic influence around the surfaces of ferreous bodies; but as they are not so familiar to observation, and often operate as a secondary cause in the production of phenomena; and in some cases even at a more remote distance from the primitive source of excitement, it is, perhaps, not to be wondered at, that their influence and laws of action, have been permitted to remain so long in concealment.

Remote and mysterious as the agency of the natural magnetism of the metal in this and other processes of exciting electricity may appear in the present infantile stage of the

science, I have much reason to suppose that such is the fact. The phenomena in magnetic electricity, as well as those in electro-magnetism, are highly favourable to the hypothesis; and I am not aware of an exception that militates directly against it. Moreover, the facility with which the *modus operandi* might be explained upon the simple principles of polar magnetic lines alone, would, I am persuaded, establish a degree of plausibility at least, not easily shaken by any counter-reasoning likely to be advanced; and the illustrations which it would be possible to bring forward in support of such an hypothesis, might possibly be the means of fixing a basis on which the theory of excitation in this curious branch of physics is eventually and permanently to be established.

The same class of remote laws apply equally to electro-magnetism as to magnetic electricity; and it would be very difficult indeed, independently of those laws, to completely harmonize with each other the phenomena displayed by the two different modes of excitation.

With regard to electro-magnetic action, the idea can hardly be said to be novel. Mr. Buxton long ago asserted that the magnetism of the conducting wire becomes polarized, and is the intermediate agent between the transmitted electric current and the magnet employed; but the illustrations which have been advanced by that gentlemen would certainly require considerable modification to establish a theory on those principles.

I have heard brought forward, as an argument against the hypothesis of magnetic polarity of the conducting wire, an experiment of Sir H. Davy's, which showed the deflection of an electric current passing through air between the charcoal points of a voltaic battery, by the presentation of a magnetic pole. Such arguments can have but very little force in discussions of this character; for the experiment develops nothing different to the generality of electro-magnetic phenomena. If an electric current be capable of rousing into activity the dormant magnetic powers of ferruginous matter, no doubt can possibly be entertained of its susceptibility of being put into motion by the energies of an already formidable polarized bar.

This is the extent of reasoning to which the experiment can be applied even under the supposition of the electric current being the immediate agent in the process of magnetizing iron or steel, and that no intervening polarization of the conducting wire is concerned in the operation; which, in fact, is no argument whatever, further than might be advanced from any other electro-magnetic experiment.

On the other hand, it might be inferred with a great deal of propriety, that if the electric current is capable of calling forth the latent magnetism of hard steel, in which it is pent up and retained with a degree of vigour which requires the greatest efforts of the exciting agent to extricate it and accomplish its polarity even to a comparatively small extent;—it is but reasonable to expect that in those metals which do not possess so exalted a degree of retention as hard steel, the same exciting agent would accomplish a polarity to a much greater extent.

This simple induction is beautifully illustrated and substantiated by demonstrable facts, by comparative experiments on soft iron and hard steel; and it was by the same mode of reasoning that I was first led to construct electro-magnets of soft iron*; since which time the practice has been pursued with more than anticipated success.

The facility of polarizing the magnetic matter, or of arranging it into active polar lines by any constant exciting force, appears to be inversely proportional to the retentive quality of the metal on which the process is performed.

The retention of magnetic polarity is displayed to the greatest extent by very hard steel. After this the retentive faculty diminishes with various grades of hardness down to soft steel; thence by gradations downwards to the softest iron, which exhibits the faculty of retaining magnetic polarity, only in a very slight degree indeed. But the facility of magnetizing those bodies, and the extent to which their polarity is exhibited, are in precisely the reverse order.

Now, as the retention of polarity appears to result from a want of facility, on the part of the metal, to re-admit the magnetic matter which the exciting agent has arranged into active polar lines on its surface and vicinal medium; and as those metals which display the retentive faculty in the greatest degree also offer the greatest resistance to the formation of those polar lines, or to the escape of the magnetic matter from its ferruginous prison;—this disposition evinced by the metal, of resisting both the egress and ingress of the magnetic matter, must necessarily arise from a natural tendency which it possesses to refuse the transmission of the magnetic element. Hence those metals which retain magnetic polarity in the highest degree may be called *inferior* magnetic conductors; and those which retain no traces of polarity after the exciting process has ceased to operate, may be called *superior* mag-

* See Transactions of the Society of arts, &c. vol. xliii.; Phil. Mag. and Annals, N. S., vol. xi. p. 194.

netic conductors, with as much propriety, and for the same reason, as similar terms are employed in electricity.

Under these considerations it will appear that hard steel is an exceedingly bad conductor of magnetism, because it offers a very great resistance to the motion of the magnetic matter. This resistance causes the process of magnetizing to become exceedingly tedious; and with very hard cast steel it very seldom terminates successfully, or to the satisfaction of the operator. Hence, in a practical point of view, it is interesting to know that magnets constructed of cast steel should never be harder than the blue temper.

Soft iron being the best ferruginous conductor of magnetism, offers a much less resistance to the flow of the magnetic matter than when in any other state. The vigorous *polar magnetic lines* are therefore speedily arranged, and to an extent of concentration never to be accomplished on the surface of very hard steel.

But the same conducting quality which gives to soft iron a facility of excitation, also gives a facility to the return of the magnetic matter into the metal when the exciting agent is withdrawn; for which reason the retention of polarity displayed by soft iron is exceedingly feeble, and easily deranged.

Hence it appears that, as far as ferruginous bodies are concerned, the vigorous retention of magnetic polarity exhibited by some of them, and the almost total absence of this quality in others, may very easily be explained upon the principles already advanced: and perhaps it would only require that we should consider copper and other non-ferruginous metals to be still better magnetic conductors than soft iron, to reconcile the sudden and total disappearance of polarity in them to the same principles, whether the exciting agent be magnetic or electric.

I have deflected a magnetic needle by an electric current traversing an ignited charcoal conductor, as was first shown by the very interesting experiments of Mr. Kemp; but as we are not aware of the total absence of the magnetic matter in charcoal, the experiment is inconclusive, any further than as an interesting fact, which has no particular bearing on the present discussion.

The energies of ferruginous electro-magnets are invariably exalted by multiplying, to a certain extent, the number of coils of conducting wire. My large electro-magnet, described in a former communication, requires twelve coils to accomplish its maximum of power (400 pounds.) The general explanation of this fact is, I believe, that one wire alone is incapable of transmitting or conducting the whole of the electric force;

and therefore a multiplicity of conducting wires becomes necessary in order that the battery may be enabled to give a full and complete display of its electric energies. And in order to accomplish this object the more completely, the extremities of all the wires are brought as close as possible to the voltaic plates. The wires of the large American magnet constructed by Professor Henry, are even soldered to the plates of the battery.

I find, however, that although an addition of coils is attended with an accession of magnetic power until a maximum of polarity is accomplished, it is by no means essential that all those wires arrive immediately at the battery. A single copper wire may intervene between the coils round the iron and the poles of the battery without deteriorating the energies of the magnet, which will still be displayed to a maximum, as decidedly as if the whole system of wires were soldered directly to the plates.

My large electro-magnet is still capable of supporting its 400 pounds, notwithstanding the electric force has to traverse six inches of bell-wire before it arrives at the coils; and also six inches more from its quitting the coils till its arrival at the other pole of the battery;—in all, twelve inches of single bell-wire. There is a limit, however, to the dimensions of the intervening wires. If they be too long or too thin, the magnet will not display its maximum of power. With pretty stout bell-wire, and the length not exceeding twelve inches, I always succeed. The battery which I employ is a single pair of metals, sufficiently small to be placed in a pint pot.

This novel aud curious fact is one of those which bears directly on the subject in question, and in a theoretical point of view is of a most interesting character. In practice, also, I find that it is exceedingly useful; giving a facility of manipulation so desirable in the management of very large electro-magnets, but which is not to be expected when all the extremities of the wires arrive immediately at the copper and zinc.

The theory of polar magnetic lines which I have advanced, requires not two magnetic fluids, nor indeed is it favourable to that doctrine: and if it be not fatal to the circulating currents of Ampère, it will at least require them to be in motion in a great variety of planes, which that distinguished philosopher never intended they should pursue. It is possible, however, that electric currents are naturally attended with magnetic polarity, independently of that which has been supposed to be excited in the wire; but it is by no means so probable that the existence of magnetic polarity is universally

due to the permanency of electric currents. Electric currents may very possibly, either directly or indirectly, magnetize the terrestrial globe; but we have no reason whatever to believe that such currents are essential to give retention of polarity to steel.

The introduction of polar magnetic lines into the theory of electro-magnetism would simplify the explanation of the phenomena, and reduce them to the principles of magnetics; and experiments may be shown in both sciences which are favourable to such a conclusion, independently of any consideration that would reconcile to identity the electric and magnetic matter.

If it can be admitted as an universal maxim in nature, that when one species of matter is impregnated with, contains, or is charged with another, the charged body must necessarily be of a grosser texture than the substance with which it is charged, or that the latter should be more subtle than the former; then it is possible that the magnetic matter, which is the most subtle we are acquainted with in nature, may insinuate itself into the pores of the electric; and the latter become charged with the former, as decidedly, under some circumstances, as a piece of iron is naturally charged with them both.

I shall not, however, on the present occasion, advance further into speculative suppositions of this kind, which, however curious they may appear in themselves, are perhaps not of much interest in the present stage of our knowledge of physical operations.

In the positions which I have advanced, I have carefully avoided every consideration that could possibly embarrass the mind, or prevent them from being understood. They would virtually, however, have been but very little affected, by taking into account the magnetism of the metal as an intermediate agent in the process of excitation; but they are much simplified by omitting those remote laws, which would be better exhibited separately, and as a distinct class, which may be admitted, or rejected, at pleasure, without affecting the calculations of the experimenter.

The paper, of which the above theory formed a part, was originally published in the London and Edinburgh Phil. Mag., vol. ii. of the present series. The remote laws of electro-magnetism, here spoken of, will he found illustrated in the following article.

L. *Application of the preceding Theory, and of the laws of Electro-magnetism, to the explication of Phenomena. By* WILLIAM STURGEON, *Lecturer on Experimental Philosophy, at the Honourable East India Company's Military Seminary, Addiscombe, &c. &c.*

1st. The most simple application of this theory is in the production of electric currents by the motions of conducting bodies in the vicinity of a bar magnet. Let O, fig. 68, plate IX. represent a transverse section of an endless metallic wire, situated in the magnetic atmosphere of the bar, N, S, whose *polar lines* are consequently some on one side, and some on the other, of that portion of the wire represented by the section O. If now, the wire O be made either to approach the bar, or to recede from it, it will have to pass through some of those exciting magnetic lines. Hence, by position 2, the electric fluid in the wire will be put into motion.

If the wire be made to *approach* the steel bar, it will then advance on those magnetic lines situated between them, and according to positions 6 and 7, the direction of its electric current will be *from* the spectator, looking at the figure, to behind the paper on which it is printed, and where the wire is supposed to be continued. But if the wire be made to *recede* from the magnetic bar, its electric fluid will be excited by the impressions of those polar magnetic lines which are *exterior* to it, and the current will flow *towards* the spectator, or in the opposite direction to the former current. It is obvious, however, that, although the currents thus produced will flow in directions the reverse of each other in the wire, and also with regard to the position of the magnetic bar, they still observe *one* and the *same* direction with reference to those magnetic lines which impel the electric fluid into motion. Precisely the same phenomena would be displayed if the wire were stationary, and the magnet put into motion.

Remark. By inspection of fig. 68, it will appear obvious that the currents will observe these directions in all cases where the advance and recession of the wire are between the extremities of the bar, and in a plane perpendicular to its axis.

2d. If the wire be kept perpendicular to the axis of the magnet and passed down the side of the latter from the upper extremity N, to its centre; the efficient magnetic lines will have the same relation to the wire, as those to the *advancing* wire in the first application; and the current will be *from* the spectator. But if the wire be continued in its downward

motion farther than the centre of the magnet, it will then advance on the efficient magnetic lines in the opposite direction to that whilst moving down the first half; and the current thus produced will be *towards* the spectator. By moving the wire in the opposite direction, or from the lower to the upper extremity of the magnet, then, because of its advancing upon the magnetic lines in precisely the same order as before whilst moving downwards from N to S, the currents thus produced, will observe the *same* directions whilst the wire passes the first and second halves of the magnet respectively. So that the current produced opposite the lower half will be *from* the spectator; that produced opposite the upper half will be *towards* the spectator. Hence this practical

Rule. If a wire be placed at the centre of a magnet, and at right angles to its axis: then if it be moved parallel to itself either towards the north or the south pole, the electric current produced in that wire will always observe *one* and the *same* direction. But if the wire be moved from either pole *towards* the centre of the magnet, the current produced will be in the *opposite* direction to the former: the *directions* of both currents being conformable to the law laid down in positions 6 and 7 of the theory. And as the same arrangement of *magnetic lines* is observed on every side of a magnet, the *rule* holds good in the motions of rings, or endless flat helices placed on the axial bar. Or, when those forms of conductors are stationary, by the motions of a magnet in their axes. In these cases the exciting impressions take place on every side of the ring, or helix.

3d. The same laws of excitation apply to the phenomena exhibited by the employment of a horse-shoe magnet as to those exhibited by that of a straight bar: and may easily be understood by an attention to what has been said respecting the latter kind. If, for instance, one portion of an endless wire were to be placed between the branches of the horse-shoe magnet represented by fig. 62, and perpendicular to its plane: then a motion of that part of the wire, parallel to itself, from the bend of the magnet towards its poles, would advance it nearly perpendicularly on those magnetic lines situated immediately between the branches: and also on those curved magnetic lines which are above and below the space between them, and whose poles are in the same direction; as may be understood by looking at fig. 63. Hence, by *positions* 6 and 7, the current produced in that part of the wire would be *from* the spectator looking at fig. 62. But if the wire were to be moved in the opposite direction, or from the poles towards the bend of the magnet, the current in the same part

of the wire would be *towards* the spectator: both currents with this magnet observing the same laws as with the straight one: and invariably referrible to the polar positions of those magnetic lines on which the wire advances.

Imagine, now, the endless wire to be a ring which will move freely on either branch of the magnet, and its motions similar to those before described. Then, although the directions of the currents and motions of the ring will still observe the same relation to each other as before; each current, excepting in that part of the ring immediately between the branches of the magnet, will *appear* to change its direction, by the motions being made on this or that particular branch; as may be understood by looking at fig. 69, where the arrows indicate the directions of *one* and the *same* current in the ring when placed on different branches of the magnet. The imagination may possibly be assisted by considering the original ring to be split, or composed of two flat rims laid side by side, and susceptible of separation, excepting on one side, where they would still be held together. These two portions of the original ring being now placed on the poles of the magnet, one on each as in fig. 69, and moved *from* the poles towards the bend, would each carry a *branch current* from that excited in the *whole* part of the original ring situated more immediately between the magnetic poles. Behind the magnet, one of these branch currents would flow towards the spectator's right, and the other towards his left: whilst the *main current* between the branches of the magnet would flow directly *from* him. Reversing the motion of the wire would be attended with the usual vicissitudes in the direction of the current.

The *apparent* anomaly exhibited by these phenomena, which is a mere deception arising from the figure of the magnet requiring those parts of the ring not immediately between its branches to be placed towards the right of the spectator in one case, and towards his left in the other, has been productive of much mystery, wherever a perfect knowledge of the fundamental laws has been wanting. The same illusion is effected by operating with the poles of a bar magnet, provided they be placed in one and the same direction (say upwards) during the time they are employed.

All that has been said about rings apply to helices of every description.

By discovering a similar illusion in M. Ampère's beautiful experiment in which a magnet rotates on its axis, I was enabled to rotate a large magnetic bar, by causing two electric currents to traverse, each half its length, from its poles to its centre, or conversely; which currents, according to the views

previously taken of the nature of the action, ought to have counteracted each other's effect.

4th. Let the bar N, S, fig. 68, be a cylinder of soft iron, and O the section of a wire placed at right angles to it. If, now, the iron bar be converted into a magnet, similar to that represented by the figure, by the application of the poles of permanent magnets at its extremities, polar magnetic lines will start from its surface on every side; and those on the right side will advance upon the wire O, and thus give the exciting impressions, and cause an electric current in the wire, whose direction will be *from* the spectator (See positions 6 and 7.). But when the permanent magnetic poles are withdrawn from the iron cylinder N, S, the electric current in the section O will be reversed; being then caused by those magnetic lines exterior to the wire, which, by rushing towards their native bar, give the exciting impressions.

To prevent circumlocution, it may be convenient to call the first-mentioned motion of the magnetic lines, the *magnetic distention*, and the latter motion of those lines, the *magnetic collapsion*. The electric currents excited by either the *expansion* or *collapsion* will continue to flow during the whole time the magnetic lines are in motion; but will cease to exist when those lines have become stationary.

By considering O the section of one side of a ring, or of one convolution of an endless helical wire, placed on the iron bar; it will be easily understood that by the *magnetic distention* the exciting magnetic lines will advance upon the *inner* surfaces of those conductors, and give impressions on every side alike: and by the *collapsion* those lines will advance upon their *outer* surfaces; again giving the exciting impressions on every side. Hence, during either a *distention* or a *collapsion*, a ring receives more exciting impressions than a straight wire, and a helix more than a ring: whilst the same law holds good in every case.

Hitherto the application of the proximate laws of magnetic-electricity appears exceedingly simple, whether the exciting magnetic lines be *permanent* or *transient*. But before we proceed to the application of those laws which are not so easily perceivable, it will be necessary that some explication of them be given in the simplest experimental process in which they are found to operate. By the proximate laws of magnetic electricity alone, we can account for the production and direction of an electric current in a helix enclosing an iron bar, by the conversion of that bar into a temporary magnet; but those laws do not furnish us with means sufficient to comprehend the converse fact, viz: That a similar current ir

the helix from any other source of electric action, would convert the iron into a magnet whose poles would be the reverse of the former. Nor do I know that this fact is much known; —certainly never attempted to be explained.

If the electric current and the ferruginous magnetic matter operated on each other by *direct* action, in the production of these phenomena, a direct reciprocity of excitation would certainly be expected: but since experience shows that this is not the case, the discovery of an intermediate agent, if any there be, becomes desirable and important. This agent has long appeared to me to be the magnetism of the conducting wire, whose laws of action I will now endeavour to develop. I shall consider the source of electric action to be a single voltaic pair: and as it is possible that some of my readers my not be acquainted with the phenomena, I shall put them in the shape of problems; and attach to each its proper solution.

Problem.—Why do parallel wires carrying electric currents in the *same* direction, attract each other? But when carrying currents in *opposite* directions repel each other?

Every electric current is productive of, and enveloped by, a magnetic atmosphere, extending outwards to various distances according to various circumstances connected with the source of the current and the nature of the conductor. The particles constituting this atmosphere, like all other magnetic particles, are north and south polar, and arranged in consecutive polar order in circular planes, concentric with the axis of the current.

Let the circle *c*, fig. 70, Plate IX., represent a transverse section of a conducting wire carrying an electric current *from* the spectator to behind the paper. Then the magnetism of the metal, and perhaps that of the surrounding medium also, will be arranged by that current in an infinite number of curve lines concentric with the boundary of the section, and extending to a considerable distance, in the manner shown by the figure.

Let, now, these magnetic lines, or the magnetic force they represent, be compounded into four *tangential* lines, as shown round *c′*, *c*, fig. 71; and let the *tangential* lines round *z*, also represent the resultants of the magnetic force surrounding the section of another current, flowing in the opposite direction to the former. Then, by contemplating the character of those magnetic lines which are the most contiguous to each other in these three sections, we find that round those wires *c*, *c′*, carrying currents in the *same* direction, the vicinal lines present *dissimilar* poles to each other: hence, by the laws of

magnetics, they will have a tendency to *approach* each other. But by observing the magnetic arrangement about those currents which flow in opposite directions, as in c', and z, it is found that *similar* poles are presented to each other by the contiguous lines situated between the wires; on which account the tendency is to recede from each other.

The magnetic lines thus *immediately* produced by an electric current are the primary or true electro-magnetic lines. Those about a soft iron bar, magnetized by electricity, being a secondary production.

Problem.—In what manner do the primary electro-magnetic lines operate on a ferruginous bar enclosed in a spiral conductor?

The electro-magnetic lines operate on the latent magnetic particles of iron in precisely the same manner as they are operated on by the magnetic lines of a permanent steel magnet; arranging those particles in regular consecutive polar order according to the laws of magnetics. Or, in the same manner as two magnetized bars would arrange themselves with each other, when permitted to move by their mutual tendency alone, which would be side by side with their poles reversed.

Let the small circles in fig. 73, Plate X, represent sections of a spiral conducting wire, enclosing a soft iron bar N S; and let the electric current through the three sections on the upper side be carried *from* the spectator; and consequently returning *towards* him through those on the lower side of the spiral. The electro-magnetic force of this spiral will call forth the latent magnetic matter of the enclosed bar, and arrange it in magnetic lines in the reverse order of those resultants in the interior of the spiral; which, in consequence of their vicinal situation are the principal ones brought into action: and by their operating in concert with each other, the magnetization of the ferruginous bar is promptly and uniformly accomplished. The short lines with cross heads on the face of the magnet, and the long lines on its upper and lower sides, show the direction of the ferreous magnetic lines with reference to the extremities of the bar, a representation of the actual distribution of those lines being omitted, to prevent confusing the figure.

Remark.—It will now be seen that the ferreous magnet is not a *direct* or true *electro-magnet* as is generally supposed: but is a magnet by a *secondary* cause, or by the action of the electro-magnetic lines of the conductor. The theory of electro-magnetic lines will also explain the curious problem presented by the fact, that ferreous magnetic electricity, and the electro-magnetism of ferreous bodies are not the reverse of each other.

The ferreous magnet N S fig. 57, would have its poles in the reverse order, if the electric current were the *immediate* cause of its production; unless one system of laws prevailed in magnetic electricity, and another system in electro-magnetism; but by recognizing the electro-magnetic lines, the phenomena, by both processes, harmonize with each other, and are in exact accordance with one and the same system of laws, which are found to prevail in the minutest and most complex ramifications of this branch of physics.

Problem.—If a wire, carrying an electric current, be approached by another (endless) wire, why is the production of a *secondary* electric current in this latter wire? Why the production of another current by the receding of the endless wire? And why do these *secondary* currents traverse the wire in reverse directions? Why, also, the transientness of these secondaries?

Let *c*, fig. 72, be a section of a conducting wire carrying an electric current *from* the spectator; and let *c′* be a section of an endless wire, placed within the magnetic atmosphere of the former; and which can be made to approach or recede from it at pleasure. When the endless wire *c′* approaches the conductor, it will advance on those magnetic lines of the latter which are situated between them, and thus become the channel of a *secondary* current, as decidedly, and for the same reason, as an electric current would be produced by its advancing on the magnetic lines of a ferreous magnet; and according to positions 6 and 7 of the theory, this *secondary* electric current will flow *towards* the spectator; or in the opposite direction to that of the primitive current. But when the endless wire recedes from the primitive conductor, it will advance on those magnetic lines *exterior* to it; and the *secondary* current being produced by the exciting impressions of those lines, will flow in the opposite direction to the former secondary, and consequently in the same direction as the primitive current in the conductor *c*.

The transientness of these secondary currents will be obvious by considering that neither of them can exist any longer than whilst the wire is in motion.

Problem.—If an endless wire be placed parallel to the conductor of a voltaic pair, a *secondary* current is produced in the endless wire at the moment of completing the battery circuit; and another *secondary* current is produced in the same endless wire when the battery current is cut off, but none during the intermediate time. Why these phenomena? Why also do these secondaries run counter to each other?

The explanation in this case will be similar to that in ap-

plication 4th. Let *c* fig. 72 represent the section of a conductor ready to carry a battery current *from* the spectator, at any moment it may be wanted; and *c′* a section of an endless wire placed close beside the former, and parallel to it. When the battery contact is made with the former wire *c*, the rushing current displaces the latent magnetic particles of this conducting wire, and arranges them in regular polar lines; which, distending on every side, some of them necessarily advance on the other wire, and produce in it a *secondary* current; which, according to positions 6 and 7 of the theory, will be in the reverse direction to the primitive battery current. When the battery current is cut off from the conductor *c*, a collapsion of its magnetic lines will take place, and those of them which are exterior to the other wire *c′* will give the exciting impressions, and the *secondary* current thus produced, will flow in the *same* direction as the primitive battery current; and consequently in the reverse direction to the former secondary.

Remark.—It will now appear very obvious that the *primitive* electric current is not the *immediate* agent in the production of the *secondary*, although it is the primary cause. Its immediate agency is productive of the arrangement and distention of magnetic lines; which lines become *stationary* as soon as their parent current flows steady and equable; and remain stationary as long as that current is uniform, but no longer. When the *primary* current is cut off, its magnetic lines *collapse* and disappear, because the cause of their existence now ceases to exist. Hence the magnetism of the conducting wire, &c., being an intermediate agent, becomes a *secondary cause* in the production of the secondary electric current. It appears, therefore, that the production of the magnetic lines by the primary current is truly *electro-magnetic*, whilst the *secondary* current, on the contrary, is a *magnetic-electrical* phenomenon. Hence, also, as the very existence of the secondary current depends upon the motion of the primitive electro-magnetic lines, no secondary can possibly be continued, when those lines have become stationary.

The explanation here given of the production and direction of secondary currents, applies to conductors of every fashion, whether they be straight or curved; helices or simple rings; for in all cases the sections *c*, *c′* may be considered as portions of any shaped conductor whatever.

Lemma.—Those effects produced by an electric current when a circuit is first completed, may be called the *initial* effects; and those produced when the current is cut off, the *terminal* effects.

Problem.—Why are the *initial* magnetic effects of an electric current greater than the *terminal?* And why are the physiological effects in the reverse order?

The *initial* magnetic effects are caused by the production and sudden distention of electro-magnetic lines on every side of the conductor: by which means the needle becomes vigorously deflected, to an extent much greater than the subsequent steady uniform arrangement of those lines will maintain it. But when the battery current is cut off, those lines immediately collapse and suffer annihilation; so that instead of deflecting the needle farther than before, they cease to support its deflection altogether. The same explanation applies to the magnetizing of ferruginous bars.

The physiological phenomena, on the contrary, being the immediate effects of electric agency, and not of the electro-magnetic, require a distinct explanation.

The *initial* electric effects can proceed from no cause but that of the original source of the current, as from the action of a voltaic pair, for instance. For although this current will be productive of magnetic lines, those lines during their formation and distention, *recede* from the current, and do not advance upon it; hence they give no exciting impressions. They are, in this instance, no *cause* of the current's existence nor of its energy, but, on the contrary, are one of its productions. Moreover, as the physiological phenomena are displayed to the greatest advantage when the conducting wire is formed into a close coil, it may be shown that the distention of the magnetic lines will tend to produce a counter current in the conductor.

Let *c c'* fig. 74, Plate X, represent transverse sections of two vicinal portions of the conducting wire, both ready to convey the current, by the *initial* impulse, *from* the spectator. The magnetic lines, by this current, will be arranged round both sections in the order seen in the figure; and during their *distention* will advance on each other's portion of the wire, giving exciting impressions for the production of a *secondary* current, which secondary, according to positions 6 and 7, would flow in the reverse order to that of the primary battery current, and the closer those sections are together the more effectual would be those exciting impressions. And as every section of the wire produces a similar distention of its magnetic lines, the tendency to produce a secondary current in the vicinal convolutions of a coil becomes greater as those convolutions are more numerous, and more closely packed together. This tendency to produce a counter current in the conductor partially neutralizes the efforts of the primary battery current. Hence the

atony in its *initial* physiological effects. Hence, also, the transient deflections of a magnetic needle situated at a remote part of the circuit, become *lessened* by having the principal part of the conducting wire formed into a compact coil. (See table of transient deflections, page 191.)

When the battery current is cut off, the electro-magnetic lines, which, during their distention tended to counteract the efforts of this current, now suddenly *collapse*, and by advancing rapidly on the moving fluid on every side of the axis of the current, give to it a new impulse in the direction of its previous motion; and the electro-momentum, thus increased, is now enabled to overcome those resistances which the battery energies alone were not capable of penetrating; and as the physiological effects are strengthened in proportion as the resistances presented by animal bodies are conquered, this class of the *terminal* effects of a current become greater accordingly. The manner of excitation by this collapsion may possibly be better understood by again looking at fig. 74, where the two vicinal sections of the coil wire are supposed to be situated within each other's electro-magnetic atmospheres; the two groups of remote polar lines c and c' being portions of those belonging to the sections c and c' respectively. When the battery connexions are cut off, a *collapsion* of the electro-magnetic lines in the coil suddenly ensues, and those represented by the group c rush upon the section c'; whilst those represented by the group c' rush upon the section c; each group producing new exciting impressions, upon the principles of magnetic electricity; and by positions 6 and 7 of the theory, these exciting impressions are productive of a momentary electric current, in the *same* direction as that originally flowing from the battery. Only two sections of the coil wire are drawn in the figure, to prevent confusion in the illustration; but it is to be understood that as the electro-magnetic atmosphere of each convolution extends to a sufficient distance to embrace several of the neighbouring convolutions, the electro-magnetic collapsion of one convolution will be productive of exciting impressions in many others which are placed near to it; on which account the electro-physiological effects are greatest when the wire is formed into a close compact coil. See experiments, commencing at page 192.

Problem.—Why are the initial *secondary* currents feebler than primitives which bring them into play? And why are the terminal secondaries in the reverse order?

A *secondary* current is the immediate production of either a distention or a collapsion of the magnetic lines of the primitive. In the first case the lines advance on the second

conductor, and produce in it a current in the reverse order to that of the primitive, as has already been shown. This *secondary* current, in its turn, becomes productive of magnetic lines, whose poles, on the adjacent side, would be in the same direction as those of the primitive magnetic lines: as will be understood by looking at *c* and *z*, fig. 71, Plate IX. The former being a section of the primitive, and the latter a section of the secondary current. Hence, the moment the *secondary* begins to flow by the impressions of the *foremost* lines of the distention from the primitive; magnetic lines from this feeble secondary would distend also, and meet those of the primitive, which had not yet arrived at the electric matter in the wire, *north* pole against *north* pole, and *south* pole against *south* pole, until the reacting poles of the secondary system of magnetic lines had counterbalanced the acting poles of the primitive system. At this period there would be a virtual pause of both systems of magnetic lines. And as the very existence of the secondary current has hitherto depended upon the distention of the magnetic lines of the primary, this pause would tend to slacken the secondary current, and even terminate its existence were its slackening not attended by a partial collapsion of its own magnetic lines, and a corresponding distention of those of the primitive; by the exciting impressions of which it would be again partially recruited until another hostile meeting of the two systems of magnetic lines had again brought all motion, excepting that of the primitive current, to a pause. This pause would be succeeded by another partial distention of the primitive magnetic lines, which would again impel the secondary current; and by similar vicissitudes would the existence of this current be continued, until the arrival of the hindermost distending magnetic lines of the primitive, which, by giving the terminal exciting impressions, would produce the final effort of the secondary, whose cause of existence having now vanished, would itself soon cease to exist. These motions and counter-motions of the magnetic lines of the *secondary*, would impede the progress of those of the primitive: and convert, as it were, their usual smooth *rapid* distention, into a comparatively *slow* pulsatory one; whose exciting impressions thus impaired, would be attended by a corresponding atony in the resulting secondary current.

With regard to the phenomena attending a *terminal* secondary and its primitive, the first part of the process is the suspension of the battery action, which will be attended by a collapsion of the primitive magnetic lines; and those of them which were situated exterior to the wire of the secondary will advance upon it and give the exciting impressions which bring

the secondary into existence. The production of this secondary will be attended with a distension of its own magnetic lines, whose poles will be arranged in the same order as those in the primitive. Those magnetic lines, of the two systems, which are situated between the wires will present poles of opposite kinds to each other, by the influence of which, the two wires, if free to move, would be drawn together; but as the wires are fixed, the magnetic lines alone will approach each other with great celerity. This attraction will be attended with a partial retardation in the subsequent part of the collapsion of the primitive's magnetic lines; and a more free distention of those of the secondary. The former effect will lessen the usual magnetic impulse on the primitive current; and the latter will tend to produce a *counter* current in the wire of the primitive. Hence, on both these accounts, the terminal effects of the primitive will be much abated, if not completely annihilated.

The secondary, on the contrary, being once brought into play, has nothing to obstruct its motion; and the free collapsion of its own magnetic lines giving it another impulse will enhance its original effects, as decidedly, and for the same reason, as a battery current is exalted when no secondary conductor is present.

The phenomena of *secondary* electric currents unquestionably present the most complex problems of any in either electro-magnetism or magnetic electricity; and I have selected those for solution which appear more difficult of explanation than any other with which I am acquainted, as a test for the correctness of the theoretical principles which I have advanced. I submit the whole to the candour of Philosophers, hoping that none will find fault, who are not perfectly prepared to improve upon what I have done.

LI. *Contributions to Electricity and Magnetism. By* Joseph Henry, *Professor of Natural Philosophy in the College of New Jersey, Princeton; late of the Albany Academy.*

Description of a Galvanic Battery for producing Electricity of different Intensities. Read before the American Philosophical Society, January 14*th*, 1835.

The following account of a Galvanic Battery, constructed under my direction for the Physical Department of the College of New Jersey, is submitted to the American Philosophi-

cal Society with the intention of referring to it in some communications which I purpose making on the subject of Electricity and Magnetism. It is hoped, however, that the arrangement and details of the instrument, in themselves, will be found to possess some interest, since they have been adopted in most cases after several experiments and much personal labour.

The apparatus is intended to exhibit most of the phenomena of Galvanism and all those of Electro-Magnetism, on a large scale, with one battery. It was constructed to illustrate the several facts of these branches of science to my class, and also to be used as a convenient instrument of research in all cases where no very great degree of intensity is required.

The several parts of this battery are not soldered together forming one permanent galvanic arrangement, but are only temporarily connected by means of moveable conductors and cups of mercury. The whole is constructed in reference to the principle well understood of producing electricity of greater or less intensity, by a change in the method of uniting the several elements with each other.

The apparatus consists of eighty-eight elements or pairs, composed of plates of rolled zinc nearly one eighth of an inch thick, nine inches wide, and twelve inches long, inserted into copper cases open at top and bottom. Eleven of these elements are suspended together from two cross pieces of wood; and the whole number is thus arranged in eight sets, of eleven in each. These are supported by the ends of the cross-pieces in a strong wooden frame, so as to be immersed in eight separate troughs; they thus form as many independent batteries, which can be used separately or together as the occasion may require. Each trough is divided into eleven cells by wooden partitions coated with cement. If one of the cells be charged with dilute acid, a single element may be excited without producing action in any other part of the battery. Each set or battery may also be lifted separately from the frame by its cross-pieces, without disturbing the other parts of the apparatus.

The elements remain stationary, while the troughs are raised to them on a moveable platform by the common application of a wheel and pinion.

The general arrangement of the whole may be seen at once by a reference to the perspective drawing, fig. 75 Plate X. *a a*, &c. represent the cross-pieces resting on the upper part of the frame of the machine; *c c* is the moveable platform.

A perspective view of one of the elements on a larger scale is given in fig. 77, *a a* are two cups of cast copper, with a broad

stem on the bottom; one soldered to the zinc plate, and the other to the copper case. The cavity in these cups is about three eighths of an inch wide, a little more than an inch long, and half an inch deep. The cups being well amalgamated and partially filled with mercury, receive the ends of the copper conductors which unite the several elements.

For the purpose of suspension, a slip of copper, *b b*, with a hole in it, is soldered to each upper corner of the copper case; these fit loosely into a mortice or narrow groove in the cross-pieces, and are secured by a pin of copper wire. When the pins are withdrawn, a single element may be removed from any part of the series, without disturbing the remainder.

The zinc plate is fastened into its copper case, without touching, by a piece of wood at each corner with a groove in it to receive the edge of the plate. The grooves in the two lower pieces of wood terminate at about a quarter of an inch from the lower end, and thus form shoulders, which prevent the plate from slipping down: while the wood itself is supported by a flange, formed by bending in the lower edges of the corner of the copper case.

There are two principal sets of connectors; the first is formed of bars of cast copper, thirteen inches long, an inch wide, and about an eighth of an inch thick. On the lower side of these are eleven broad projections, which fit loosely into a row of cups on the plates of zinc or copper. Fig. 78 represents one of these connectors, with a thimble soldered on the upper side, for the purpose of attaching a conductor, which may serve as a pole.

There are two of these for each of the eight batteries, and when in their places, one unites all the zinc, and the other all the copper, so that the battery becomes a calorimotor of a single element or pair. If with this arrangement the several batteries be connected, zinc to zinc, and copper to copper, by conductors reaching from one to the other, the whole apparatus of eighty-eight elements becomes a large calorimotor of a single pair; but if the copper of the first be united to the zinc of the second, and so on, it then forms a calorimotor of eight elements, and by a simple change may be reduced to one of four, or of two elements.

The other set of connectors consists of short pieces of thick copper plate, the ends of which are bent down at right angles, so as to dip into the cups of mercury; they connect the copper of one element with the zinc of the next. Ten of these, intended to unite the elements of one battery, are shown in fig. 79. They are attached crosswise to a slip of harness leather, which, by its pliability, permits them to fit loosely into

the cups, while it enables the whole set to be removed as one piece. When these connectors are in their places, and the several batteries united, the copper pole of the one, with the zinc pole of another, and so on, the whole series forms a deflagrator of eighty-eight elements.

The different arrangements of the several connectors will be readily understood by a reference to the plate, fig. 76, which exhibits one half of the whole apparatus arranged as a deflagrator of forty-four elements, and the other half as a calorimotor of four pairs. By closely inspecting the drawing, it will be seen that the connexion in the upper half of the figure is from the copper of the first element to the zinc of the next, and so on through the entire series of forty-four elements. In the lower half the union of copper and zinc takes place only between the poles of the different batteries; the several elements of which are united so as to act as one plate of copper and one of zinc. The four batteries therefore will act together as a calorimotor of four elements. The arrangement, as given in the drawing, is intended to illustrate by one figure the two sets of connectors; but such an arrangement becomes interesting in practice in determining the effect of the conjoined actions of batteries producing electricity of different intensities.

The circuit of the connexions as given in the figure is complete except at *a b*; the two plates at this point form the poles of the battery. A set of poles, however, may be formed at any other point of the circuit, by making an interruption at that place. In the same way two or more sets may be formed. It furnishes an interesting and instructive experiment to place a pair of large decomposing plates at *a b* and another at *c d*. When only one of these is plunged into a saline solution, the circuit being interrupted at the other pair, no effect is produced; but as soon as this other is plunged into a similar solution, a copious decomposition simultaneously takes place at both. Also the contemporaneous action in each element of the battery is pleasingly shown by placing at the same time several large magnetic needles on the different parts of the apparatus. These instantly change their direction when the second pair of decomposing plates touch the solution.

At first sight it might be supposed that there would be some difficulty in entering the several plates into their respective cells, but this is obviated by the precise movement of the platform on which the troughs stand. Its horizontal position is adjusted by four screws (*c c* fig.), and its corners slide in grooves in the upright posts of the large frame. Besides this, when the plates are once entered, they are not required to be

entirely withdrawn from the cells until the end of the series of experiments; since the acid descends as the plates are withdrawn, and finally fills but little more than three-fourths of the capacity of the cells. When a plate accidentally catches on the side of the cell, the battery to which it belongs is gently raised in its place and the plate adjusted.

This apparatus readily furnishes the means of making comparative experiments on the difference produced by partial and perfect insulation. When no higher degree of intensity is required than that afforded by eight pairs of plates, perfect insulation is obtained by the eight separate troughs. In higher degrees of intensity the partitions in the troughs furnish the means of perfectly insulating forty-eight of the elements: this is effected by simply charging with acid every other cell in each of the troughs, and connecting the corresponding element by conductors, which pass over the intermediate elements without touching them; with this arrangement we have six cells in each trough separated from one another by a cell without acid, or in effect by a stratum of air. For comparison with these a set of troughs has been constructed without partitions.

The want of perfect insulation is not very perceptible in the common experiments of the deflagration of large and perfect conductors; but where the decomposition of a liquid is attempted, or the battery required to act on a small or imperfect conductor, the loss of power is very great, the apparatus partially discharging itself through its own liquid, and the intensity at the poles does not increase with a short interruption of the current.

There is also considerable loss on account of imperfect insulation even in the case of low intensity, and when the poles are connected by a perfect conductor. In one experiment with an arrangement of five pairs, and the poles united by a conductor composed of thirty strands of copper bell wire, each forty feet long, the loss was found to be at least one seventh, as measured by the quantity of zinc surface required to be immersed in order to produce the same magnetic effect. I would infer from this that the most perfect of all Dr. Hare's ingenious galvanic arrangements is that in which the elements dip into separate glass vessels, as this combines perfect insulation with the power of instantaneous immersion.

A variety of experiments have been made during the past year with this instrument on several points of Galvanism and Electro-Magnetism, which will be communicated to the Society as soon as my engagements will permit me to repeat and arrange them for publication.

LII. *Contributions to Electricity and Magnetism. By* Joseph Henry, *Professor of Natural Philosophy in the College of New Jersey, Princeton; late of the Albany Academy.*

It can hardly fail to be gratifying to our readers, to have immediate access to a description of Professor Henry's original experiments, by which he obtained electric shocks, and other interesting phenomena, by the employment of a single voltaic pair; and it gives us great pleasure in having this opportunity of placing the following authorized account of them (from printed papers just received) in the "Annals of Electricity, &c." for their perusal.

The principal facts of the paper on the influence of a Spiral Conductor were communicated to the American Philosophical Society on the 16th of January, 1835, and an abstract ordered to be published, as will be seen by the following extract from the March No. of the Franklin Journal. Edit.

TO THE COMMITTEE ON PUBLICATIONS.

Gentlemen,—The American Philosophical Society, at their last stated meeting, authorized the publication of the following abstract of a verbal communication made to the Society by Professor Henry, on the 16th of January last. A memoir on this subject has been since submitted to the Society containing an extension of the subject, the primary fact in relation to which was observed by Professor Henry as early as 1832, and announced by him in the American Journal of Science. Mr. Faraday having recently entered upon a similar train of observations, the immediate publication of the accompanying is important, that the prior claims of our fellow countryman may not be overlooked.

Very respectfully yours,

A. D. BACHE,

One of the Secretaries Am. Phil. Soc.

Philadelphia, Feb. 7th, 1835.

Extract from the proceedings of the stated meeting of the American Philosophical Society, January 16, 1835.

[*Here follows the abstract.*]

On the Influence of a Spiral Conductor in increasing the Intensity of Electricity from a Galvanic Arrangement of a Single Pair, &c. Read before the American Philosophical Society, February 6th, 1835.

In the American Journal of Science for July 1832, I announced a fact in Galvanism which I believe had never before been published. The same fact, however, appears to

have been since observed by Mr. Faraday, and has lately been noticed by him in the November number of the London and Edinburgh Journal of Science for 1834.

The phenomenon as described by me is as follows. "When a small battery is moderately excited by diluted acid, and its poles, terminated by cups of mercury, are connected by a copper wire not more than a foot in length, no spark is perceived when the connexion is either formed or broken; but if a wire thirty or forty feet long be used instead of the short wire, though no spark will be perceptible when the connexion is made, yet when it is broken by drawing one end of the wire from its cup of mercury, a vivid spark is produced. If the action of the battery be very intense, a spark will be given by a short wire; in this case it is only necessary to wait a few minutes until the action partially subsides, or no more sparks are given; if the long wire be now substituted, a spark will again be obtained. The effect appears somewhat increased by coiling the wire into a helix; it seems also to depend in some measure on the length and thickness of the wire. I can account for these phenomena only by supposing the long wire to become charged with electricity, which, by its reaction on itself, projects a spark when the connexion is broken."*

The above was published immediately before my removal from Albany to Princeton, and new duties interrupted, for a time, the further prosecution of the subject. I have, however, been able during the past year to resume in part my investigations, and among others, have made a number of observations and experiments which develope some new circumstances in reference to this curious phenomenon.

These, though not as complete as I could wish, are now presented to the Society, with the belief that they will be interesting at this time on account of the recent publication of Mr. Faraday on the same subject.

The experiments are not given in the precise order in which they were first made, but in that which I deem best suited to render them easily understood; they have, however, been repeated for publication in almost the same order in which they are here given.

1. A galvanic battery, consisting of a single plate of zinc and copper, and exposing one and a half square feet of zinc surface, including both sides of the plate, was excited with diluted sulphuric acid, and then permitted to stand until the intensity of the action became nearly constant. The poles

* Silliman's Journal, vol. 22, page 408.

connected by a piece of copper bell wire of the ordinary size and five inches long, gave no spark when the contact was broken.

2. A long portion of wire, from the same piece with that used in the last experiment, was divided into equal lengths of fifteen feet, by making a loop at each division, which could be inserted into the cups of mercury on the poles of the battery. These loops being amalgamated and dipped in succession into one of the cups while the first end of the wire constantly remained in the other, the effect was noted. The first length, or fifteen feet, gave a very feeble spark, which was scarcely perceptible. The second, or thirty feet, produced a spark a little more intense, and the effect constantly increased with each additional length until one hundred and twenty feet were used; beyond this there was no perceptible increase; and a wire of two hundred and forty feet gave a spark of rather less intensity. From other observations I infer, that the length necessary to produce a maximum result, varies with the intensity of the action of the battery, and also with its size.

3. With equal lengths of copper wire of unequal diameters, the effect was greater with the larger: this also appears to depend in some degree on the size of the battery.

4. A length of about forty feet of the wire used in experiments first and second, was covered with silk and coiled into a cylindrical helix of about two inches in height and the same in diameter. This gave a more intense spark than the same wire when uncoiled.

5 A ribbon of sheet copper nearly an inch wide and twenty-eight and a half feet long, was covered with silk, and rolled into a flat spiral similar to the form in which woollen binding is found in commerce. With this a vivid spark was produced, accompanied by a loud snap. The same ribbon uncoiled gave a feeble spark, similar in intensity to that produced by the wire in experiment third. When coiled again the snap was produced as at first. This was repeated many times in succession, and always with the same result.

6. To test still farther the influence of coiling, a second ribbon was procured precisely similar in length and in all other respects to the one used in the last experiment. The effect was noted with one of these coiled into a flat spiral and the other uncoiled, and again with the first uncoiled and the second coiled. When uncoiled, each gave a feeble spark of apparently equal intensity; when coiled, a loud snap. One of these ribbons was next doubled into two equal strands, and then rolled into a double spiral with the point of doubling at

the centre. By this arrangement the electricity, in passing through the spiral, would move in opposite directions in each contiguous spire, and it was supposed that in this case the opposite actions which might be produced would neutralize each other. The result was in accordance with the anticipation: the double spiral gave no spark whatever, while the other ribbon coiled into a single spiral produced as before a loud snap. Lest the effect might be due to some accidental touching of the different spires, the double spiral was covered with an additional coating of silk, and also the other ribbon was coiled in the same manner; the effect with both was the same.

7. In order to increase if possible the intensity of the spark while the battery remained the same, larger spirals were applied in succession. The effect was increased until one of ninety-six feet long, an inch and a half wide and weighing fifteen pounds, was used. The snap from this was so loud that it could be distinctly heard in an adjoining room with the intervening door closed. Want of materials has prevented me from trying a larger spiral conductor than this, but it is probable that there is a length which, with a given quantity and intensity of galvanism, would produce a maximum effect. When the size of the battery is increased, a much greater effect is produced with the same spiral. Thus when the galvanic apparatus described in the first article is arranged as a calorimotor of eight pairs, the snap produced on breaking contact with the spiral last described, resembled the discharge of a small Leyden jar highly charged.

8. A handle of thick copper was soldered on each end of the large spiral at right angles to the ribbon, similar to those attached to the wires in Pixii's magneto-electric machine for giving shocks. When one of these was grasped by each hand and the contact broken, a shock was received which was felt at the elbows, and this was repeated as often as the contact was broken. This shock is rather a singular phenomenon, since it appears to be produced by a lateral discharge, and it is therefore important to determine its direction in reference to the primary current.

9. A shock is also received when the copper of the battery is grasped by one hand, and the handle attached to the copper pole of the ribbon with the other. This may be called the direct shock, since it is produced by a part of the direct current. It is, however, far less intense than that produced by the lateral discharge.

10. When the poles were joined by two coils connected by a cup of mercury between them, a spark was produced by

breaking the circuit at the middle point, and when a pair of platina wires was introduced into the circuit with the large coil and immersed in a solution of acid, decomposition took place in the liquid at each rupture of contact, as was shown by a bubble of gas given off at each wire. It must be recollected that the shocks and the decomposition here described were produced by the electricity from a single pair of plates.

11. The contact with the poles of the battery and the large spiral being broken in a vessel containing a mixture of hydrogen and atmospheric air, an explosion was produced.

I should also mention that the spark is generally attended with a deflagration of the mercury, and that when the end of the spiral is brought in contact with the edge of the copper cup or the plate of the battery, a vivid deflagration of the metal takes place. The sides of the cup sometimes give a spark when none can be drawn from the surface of the mercury. This circumstance requires to be guarded against when experimenting on the comparative intensities of sparks from different arrangements. If the battery formerly described (fig. 75 Plate X.) be arranged as a calorimotor, and one end of a large spiral conductor be attached to one pole, and the other end drawn along the edge of the connector, fig. 79, a series of loud and rapid explosions is produced, accompanied by a brilliant deflagration of the metal, and this takes place when the excitement of the battery is too feeble to heat to redness a small platina wire.

12. A number of experiments were made to determine the effect of introducing a cylinder of soft iron into the axis of the flat spiral, in reference to the shock, the spark, &c.; but no difference could be observed with the large spiral conductor; the effect of the iron was merged in that of the spiral. When, however, one of the smaller ribbons was formed into a hollow cylindrical helix of about nine inches long, and a cylinder of soft iron an inch and a half in diameter was inserted, the spark appeared a little more intense than without the iron. The obliquity of the spires in this case was unfavourable to their mutual action, while the magnetism was greater than with the flat spiral, since the conductor closely surrounded the whole length of the cylinder.

I would infer, from these experiments, that some effects heretofore attributed to magneto-electric action are chiefly due to the reaction on each other of the several spires of the coil which surround the magnet.

13. One of the most singular results in this investigation was first obtained in operating with the large galvanic battery

(fig. 76 Plate X). The whole instrument was arranged as a calorimotor of eight pairs, and a large spiral conductor introduced into the circuit at *c d*, while a piece of thick copper wire about five inches long united the poles at *a b*. In this state an explosion or loud snap was produced, not only when the contact was broken at the spiral, but also when one end of the short wire at the other extremity of the apparatus was drawn from its cup. All the other short moveable connectors of the battery gave a similar result. When the spiral was removed from the circuit and a short wire substituted, no effect of the kind was produced. From this experiment it appears that the influence of the spiral is exerted through at least eight alternations of zinc, acid, and copper, and thus gives to a short wire at the other extremity of the circuit the power of producing a spark.

14. The influence of the coil was likewise manifest when the zinc and copper plates of a single pair were separated from each other to the distance of fourteen inches in a trough without partitions, filled with diluted acid. Although the electrical intensity in this case must have been very low, yet there was but little reduction in the apparent intensity of the spark.

15. The spiral conductor produces, however, little or no increase of effect when introduced into a galvanic circuit of considerable intensity. Thus when the large spiral used in experiment seventh, eighth, &c. was made to connect the poles of two Cruikshank's troughs, each containing fifty-six four-inch plates, no greater effect was perceived than with a short thick wire: in both cases in making the contact a feeble spark was given, attended with a slight deflagration of the mercury. The batteries at the same time were in sufficiently intense action to give a disagreeable shock. It is probable, however, that if the length of the coil were increased in some proportion to the increase of intensity, an increased effect would still be produced.

In operating with the apparatus described in the last experiment, a phenomenon was observed in reference to the action of the battery itself, which I do not recollect to have seen mentioned, although it is intimately connected with the facts of Magneto-Electricity, as well as with the subject of these investigations, viz. When the body is made to form a part of a galvanic circuit composed of a number of elements, a shock is, of course, felt at the moment of completing the circuit If the battery be not very large, little or no effect will be perceived during the uninterrupted circulation of the galvanic current; but if the circuit be interrupted by breaking the

contact at any point, a shock will be felt at the moment, nearly as intense as that given when the contact was first formed. The secondary shock is rendered more evident, when the battery is in feeble action, by placing in the mouth the end of one of the wires connected with the poles; a shock and flash of light will be perceived when the circuit is completed, and also the same when the contact is broken at any point, but nothing of the kind will be perceived in the intermediate time, although the circuit may continue uninterrupted for some minutes. This I consider an important fact in reference to the action of the voltaic current.

The phenomena described in this paper appear to be intimately connected with those of Magneto-Electricity, and this opinion I advanced with the announcement of the first fact of these researches in the American Journal of Science. They may, I conceive, be all referred to that species of dynamical *Induction* discovered by Mr. Faraday, which produces the following phenomenon, namely: when two wires, A and B, are placed side by side, but not in contact, and a voltaic current is passed through A, there is a current produced in B, but in an opposite direction. The current in B exists only for an instant, although the current in A may be indefinitely continued; but if the current in A be stopped, there is produced in B a second current, in an opposite direction however to the first current.

The above fundamental fact in Magneto-Electricity appears to me to be a direct consequence of the statical principles of "Electrical *Induction*" as mathematically investigated by Cavendish, Poisson, and others. When the two wires A and B are in their natural state, an equilibrium is sustained by the attractions and repulsions of the two fluids in each wire; or, according to the theory of Franklin and Cavendish, by the attractions and repulsions of the one fluid, and the matter of the two wires. If a current of free electricity be passed through A, the natural equilibrium of B will be disturbed for an instant, in a similar manner to the disturbance of the equilibrium in an insulated conductor, by the sudden addition of fluid to a contiguous conductor. On account of the repulsive action of the fluid, the current in B will have an opposite direction to that in A; and if the intensity of action remains constant, a new state of equilibrium will be assumed. The second state, however, of B may perhaps be regarded as one of tension, and as soon as the extra action ceases in it, the fluid in B will resume its natural state of distribution, and thus a returning current for an instant be produced.

The action of the spiral conductor in producing sparks, is

but another case of the same action; for since action and reaction are equal and in contrary directions, if a current established in A produces a current in an opposite direction in B, then a current transmitted through B should accelerate or increase the intensity of a current already existing in the same direction in A. In this way the current in the several successive spires of the coil may be conceived to accelerate, or to tend to accelerate each other; and when the contact is broken, the fluid of the first spire is projected from it with intensity by the repulsive action of the fluid in all the succeeding spires.

In the case of the double spiral conductor, in experiment sixth, the fluid is passing in an opposite directiôn; and according to the same views, a retardation or decrease of intensity should take place.

The phenomenon of the secondary shock with the battery, appears to me to be a consequence of the law of Mr. Faraday. The parts of the human body contiguous to those through which the principal current is passing, may be considered as in the state of the second wire B; when the principal current ceases, a shock is produced by the returning current of the natural electricity of the body.

If this explanation be correct, the same principle will readily account for a curious phenomenon discovered several years since by Savary, but which I believe still remains an isolated fact. When a current is transmitted through a wire, and a number of small needles are placed transverse to it, but at different distances, the direction of the magnetic polarity of the needles varies with their distance from the conducting wire. The action is also periodical; diminishing as the distance increases, until it becomes zero; the polarity of the needles is then inverted, acquires a maximum, decreases to zero again, and then resumes the first polarity; several alternations of this kind being observed.* Now this is precisely what would take place if we suppose that the principal current induces a secondary one in an opposite direction in the air surrounding the conductor, and this again another in an opposite direction at a great distance, and so on. The needles at different distances would be acted on by the different currents, and thus the phenomena described be produced.

The action of the spiral is also probably connected with the fact in common electricity called the lateral discharge: and likewise with an appearance discovered some years since

* Cumming's Demonferrande, page 247; also Edinburgh Journal, October 1826.

by Nobili, of a vivid light, produced when a Leyden jar is discharged through a flat spiral.

The foregoing views are not presumed to be given as exhibiting the actual operation of nature in producing the phenomena described, but rather as the hypothesis which have served as the basis of my investigations, and which may farther serve as formulæ from which to deduce new consequences to be established or disproved by experiment.

Many points of this subject are involved in an obscurity which requires more precise and extended investigation; we may, however, confidently anticipate much additional light from the promised publication of Mr. Faraday's late researches in this branch of science.

LIII. *Method of increasing Shocks, and Experiments with Prof. Henry's apparatus for obtaining sparks and shocks from the Calorimoter. By* C. G. Page.*

Salem, May 12, 1836.

To Professor Silliman,

Dear Sir,

I have lately constructed an apparatus for obtaining shocks from the Calorimoter, which has furnished some curious results; and as you may, perhaps, deem them worthy of publication, I send you herewith a sectional drawing of the apparatus, with a description.

Fig. 80, Plate X. represents a section of an apparatus for obtaining shocks from the Calorimotor. The coil of copper ribbon, contained in the box B, Z, is 220 feet long, an inch wide, and has but four solderings or joints throughout its length. The separate lengths of 55 feet each are cut from single sheets of copper. This is easily done by cutting the alternate strips within half an inch to the edge of the sheet, and then bending them one upon the other, to bring them in the same line of length; in this way the integrity of the circuit is better preserved than by numerous solderings. The ribbon is wound with single strips of list intervening. On five of the coils at distances indicated by the figure, are soldered strips of copper which pass through the cover of the box and are then bent down to receive the thimbles for the mercury. This forms a convenient arrangement, as the mercury cups are easily emptied by straightening the copper strips. *t* represents the copper tube with a curved strip of copper soldered to its

* From Silliman's Journal, for Oct., 1836. This number of Professor Silliman's excellent Philosophical Journal, did not arrive in London till after Christmas last.

extremity for dipping into the mercury cups. For the sake of brevity in detailing the experiments, instead of the copper tube of right or left hand, merely the words right and left hand will be used: and by the abbreviation, neg. con. and pos. con. will be understood the strips of copper connecting the cups with the negative and positive cups of the Calorimotor.

On putting the pos. con. into cup 1, and the neg. con. into cup 2, a bright spark and sharp snap are produced, when either of the connectors is raised from its cup. When the neg. con. is raised from cup 3, the spark is more brilliant than the last, accompanied with a louder snap.

When the neg. con. is raised from cup 4, the spark is more voluminous, but not so intense as the last named, nor is the snap so loud.

When the neg. con. is raised from cup 5, the spark is still less bright, and the snap less loud.

When the neg. con. is raised from cup 6, (220 feet) the spark and snap are both feeble, even when compared with those given by cup 3. It would seem then from these results, that the limit of intensity is attained at cup 4, which gives a length of 110 feet; but this inference is somewhat weakened by the following facts. The shocks by no means obey the same law; the maximum being obtained by immersing the copper tubes in cups 6 and 1. For convenience of arrangement, suppose the positive connector is in cup 1, and the right hand in cup 1. The left hand is to pass along with the neg. con. into cups 2, 3, 4, 5, and 6, and as con. is raised from these cups successively, the shock increases, and from cup 6, is a maximum with this apparatus. It will be seen from this, that from cup 4 to 6, the shock is inversely as the spark, while in the first half of the coil, it is in the same ratio. It may be well to mention here, that I found if the surface of the mercury, where the contact is broken, be covered with water, the shock is very much increased. The rationale I am unable to give, but such is the fact. This augmentation does not take place at every rupture of contact, but is best attained by striking the connector against the bottom of the cup and quickly raising it. The shock is also increased by covering the mercury with naphtha and the mercury appeared to be oxidized, the naphtha soon growing turpid.

The next results to be stated are still more curious, and according to the received theories of electro-motion, difficult to explain. The pos. con. and right hand are still in cup 1.

The neg. con. in cup 2, and the left hand in cup 3, the shock is now stronger than when the left hand was in cup 2 with the connector, and goes on increasing as the left hand is carried into cups 4, 5, and 6, in succession.* Let now the pos. con. and right hand remain in cup 1, place the neg. con, in cup 3, and the left hand in cup 4, the shock goes on increasing as before, and when the left hand arrives at cup 6, the shock is as strong as that obtained from the whole coil (220 feet), while the actual circuit from positive to negative is only 80 feet. Let pos. con. and right hand remain in cup 1, put the neg. con. in cup 4, and left hand in cup 5, the shock is now as strong as when the whole coil is in the circuit, and when the left hand is in cup 6 the shock is stronger than can be obtained from the apparatus in any other way. These last results prove that the real maximum as indicated by the shock, is given by the direct circuit from positive to negative, through half the coil, with the lateral co-operation of the other half.

Thus much being known, we might reasonably expect that while the connectors are in the extreme cups 1 and 6, we should obtain shocks from any two intermediate cups, and this I found to be the case; but, contrary to expectation, I obtained shocks from cups entirely without the actual circuit. For instance, the pos. con. in cup 1, neg. in 3, right hand in 4, and left hand in 6. In this case the shock was slight; but by thrusting needles into the thumb and fore finger of the left hand, and immersing the needles in cups 4 and 6, the shock was extremely painful.

Again. Solder the copper tube of the left hand to the neg. con., put the pos. con. and right hand into cup 1. When the end of the neg. con. is raised from cup 4, no shock is felt, but when the other end is raised from the cup on the battery, a shock is felt. Other things remaining the same, carry the right hand from cup 1, out of the direct circuit to cup 6. Nearly the reverse of the last named phenomena takes place. A strong shock is felt when the end of the neg. con. is raised from cup 4, and a weaker one when the other end is raised from the cup on the battery. This experiment appears still more striking, when the right hand is carried into the same cup with the neg. con. cup 4: a shock is felt, although the distance by the direct circuit from hand to hand is only about eight inches. Having detached the copper tube from the connector, put the pos. con. in cup 1, the neg. con. in cup 4, the right hand in cup 4, and the left hand in the neg. cup on the bat-

* An assistant is necessary to make the immersion of the connectors.

tery. It is immaterial now which end of the neg. con. is raised, both producing a shock. If the right hand is now carried to cup 6, the shock is a maximum.

A direct shock cannot be obtained from this instrument. To test this, I passed fine needles deep into the thumb and fore-finger of the left hand, and immersed them in cup 6 and the negative cup on the battery, the positive con. being in cup 1; no shock was felt on breaking or making circuit.

If a file or rasp be inserted into either of the cups and the connector drawn across it, the shocks become insupportable from the rapidity of succession. The scintillations from the file in this case are very beautiful, being by far the most brilliant and copious in cup 4. Very pleasing effects are produced by breaking the circuit with a revolving spur wheel. A little spur wheel of copper is so made that, in revolving, one spur shall leave the mercury before the next touches. In this way a rapid succession of sparks and detonations are obtained. If bits of silver leaf are hung upon the spurs as the wheel revolves, the combustion of the silver leaf is very vivid, burning with its peculiar emerald light. The shocks produced while the wheel is revolving are very disagreeable.

The decomposition of water was easily effected by breaking the circuit under its surface with two clean strips of copper. On using two small platinum wires, they adhered as with a deflagrator.

The coil was tried with a two-quart Leyden jar, and shocks were obtained from cups entirely without the direct circuit. I refrain from stating other results with the Leyden jar, as they must be rendered somewhat equivocal, by the imperfect insulation of the coils.

It may also be worth mention, that by using the needles as before, I obtained with this apparatus, shocks from a single pair of plates only four square inches (single surface). We have then in this instrument a battery by itself, from which shocks of all grades can be obtained, and in cases of the medical application of Galvanism, it must prove far more convenient than the ordinary methods.

Postscript.

Salem, June 8, 1836.

One of the most pleasing experiments with the coil, is breaking the circuit with a revolving spur wheel. In former experiments, I produced the revolution of the wheel with a string, as in the wheel tinder box, having failed to effect it with a magnet. But I have since invigorated my calorimotor, by removing and cleaning the zinc plates, and a small horse-

shoe magnet is now sufficient to produce rapid revolutions, with the most brilliant results. The circuit in this case is terminated in cup 2, as the rotations diminish in proportion to the length of the coil used. The wheel is fitted with a wooden stand and trough, precisely as for magnetic rotation. The deflagration of the mercury is extremely vivid, giving copious fumes. If the experiment is performed in a dark room, it exhibits in a superb manner the well known optical illusion, of a wheel in rapid motion appearing to be at rest. As the wheel is illuminated by a rapid series of sparks, it does not appear to be exactly at rest, but exhibits a quick vibratory movement. I have before alluded to the nature of the shocks given by the wheel, but with this self-regulating apparatus, an assistant can be dispensed with, and shocks of any duration and degree can be obtained, by immersing the copper handles as before directed. The strongest shock being obtained by immersing the copper handles in cup 6, and the negative cup of the battery. This last experiment is difficult to explain. The left hand being in cup 6, it is immaterial whether the right hand is carried to the positive or negative cup on the battery: a strong shock is felt in both cases, but that from the negative cup is somewhat stronger, and is the real maximum, if the circuit terminates with half the coil.

Explanation of the Phenomena, &c.

Mr. Page's variations of Professor Henry's original experiments are very important; but as that gentleman has not ventured an explanation of the cause of the phenomena, it may perhaps be interesting to many of our readers to know that the whole may be explained upon the theoretical principles already placed before them. In every instance the phenomena may be traced to the *collapsion* of the electro-magnetic lines. In some instances the phenomena proceeded from a *primitive* current; in others, from a *secondary* current; and in others from both *primitive* and *secondary*.

When the battery circuit embraced portions of the coil between 1 and 2;—1 and 3;—1 and 4; the remaining part of the coil, as far as the left hand enclosed it, became the circuit for a secondary current. When the battery circuit embraced half of the coil; or from 1 to 4, the other half, when the left hand was in 6, was traversed by a secondary current, every time the battery connexions were broken. When the battery circuit embraced the coil from 1 to 3, and the hands in 4 and 6; the shock felt was entirely from the secondary current of that part of the coil immediately between the hands.

At the time I was making my first course of experiments on this subject, I had not investigated the doctrine of secondary electric currents; on which account I was quite unprepared to arrange my coils of wire to the best advantage for producing effects; hence it was that I did not succeed in producing greater shocks from the two coils A and B, fig. 16, than from one of them alone. Having subsequently, however, succeeded in tracing the whole phenomena which secondaries display, to the influence of electro-magnetic excitement, the failure has become sufficiently obvious:—the coils are not within the influence of each other. I now obtain shocks from secondaries alone, by the employment of the same wires; placing them side by side, from one end to the other in one compact coil. No iron is used. The ends of one wire are connected with the battery, charged with salt water; the ends of the other wire are immersed in two cups of salt water, one in each. The fingers being placed in one cup, and the thumb of the same hand in the other, a shock is felt every time the battery circuit is broken. Decompositions are also accomplished by those *secondary* currents.

W. STURGEON.

LIV. *On the best method of making an electro-magnet for electrical purposes, and on the vast superiority of the electric power of the electro-magnet, over the electric power of the common magneto-electric machine. By the* REV. N. J. CALLAN, *Professor of Natural Philosophy, in the R. C. College, Maynooth.**

As often as the communication between the helix of an electro-magnet and the voltaic battery is broken, an electric current is made to flow through the helix in a direction opposite to that of the current from the battery. The power which produces the former current, I call the electric power of the electro-magnet: the attraction of the magnet for iron, I call its magnetic power.

The electric current excited in the helix of an electro-magnet on breaking communication with the battery, is capable of producing all the effects of the most powerful galvanic battery,—the electro-magnetical effects, the ignition and fusion of the metals, the shock, and decomposition. The magnetic effects and ignition depend more on the quantity than on the intensity of electricity. The shock and decomposition depend principally on electric intensity.

* Communicated by the Author.

I shall now detail the various circumstances which influence the intensity of the electric current excited in the magnetic helix.

The intensity of the electric current produced in the helix of an electro-magnet, on breaking communication with the battery, depends on six things:—first, on the magnetic power of the electro-magnet; secondly, on the length of the helix; thirdly, on the thinness of the wire of which the helix is formed; fourthly, on the inverse distance between the helix and iron of the magnet: fifthly, on the manner in which the wire is coiled on the iron bar; and, sixthly, on the insulation of each coil of wire from the other coils.

To ascertain the dependence of the intensity of the electric current excited in the helix, on the magnetic power of the electro-magnet, I magnetized the same bar of iron to different degrees, and in a great variety of ways. First, by a pair of small plates, and then by a pair of large plates. Secondly, by several pairs of large plates, and afterwards by several pairs of small plates. Thirdly, by passing the voltaic current round the bar from the same plates through wires of different thicknesses, of different lengths, and at different distances from the iron. In all these cases, the intensity of the electric current produced in the helix, on breaking communication with the battery, increased or decreased according as the magnetic power increased or decreased, and appeared to increase in proportion to the magnetic power. I also compared the intensities of electric currents excited in the helices of different electro-magnets, and found that (cæteris paribus) the magnet which exhibited the strongest attraction for the same bar of iron, produced in its helix the most intense electric current. From these results, I think I am warranted in concluding that the intensity of the electric current (all other things on which it depends being given) varies as the magnetic power of the electro-magnet.

Secondly, the intensity of the electric current excited in the magnetic helix, on breaking communication with the battery, increases with the length of the helix. I have taken shocks from wires of different lengths coiled on the same iron bar, and have always found that (when the magnetic power was given) the strongest shock was obtained from the longest wire. The shock increases with the length of the wire, until the coils are at such a distance from the iron, that its magnetic power is incapable of producing an electric state in them.

Thirdly, the intensity of the electric current produced in the helix, increases very rapidly with the thinness of the wire. For, if two wires of equal length, but of different thickness,

be coiled on an electro-magnet of given magnetic power, at equal distances from the iron, it will be found that the shock given by the thin wire, on breaking communication with the battery, is far stronger than the shock from the thick wire. The shock appears to increase in a higher ratio than the inverse diameter of the wires.

Fourthly, the intensity of the electric current increases with the inverse distance between the helix and iron bar. I coiled on an iron bar three wires of nearly equal length and thickness, one over the other, and left their ends projecting. The iron bar was magnetized by passing the voltaic current through each wire successively; the shock was also taken from each wire successively. But, whether the electric current from the battery was sent through the first, second, or third wire, the shock from the first wire was, in all cases, greatest, and the shock from the third wire was, in all cases, least. Hence, the shock increases with the inverse distance between the helix and iron bar. I have not ascertained the rate of increase: but I have reason to think that the increase of electric intensity depends more on the distance of the helix from the surface, than on its distance from the axis of the iron bar.

Fifthly, the intensity of the electric current depends on the direction in which the wire is coiled on the iron bar. If the wire be coiled irregularly, or in a direction oblique to the axis of the magnet, there will be a great loss of electric intensity. The highest intensity will be obtained when the wire is coiled as nearly as possible at right angles to the axis of the magnet, and when each coil is as close as possible to the adjoining coils.

Sixthly, the intensity of the electric current excited in the magnetic helix depends on the insulation of each coil from all the other coils. However great the magnetic power may be, however long or fine the wire, however small its distance from the iron, and however perfect the coiling, the intensity of the electric current will be but trifling if the insulation be bad. When high intensity is sought for, the covering of the wire will not be sufficient to insulate each coil from all the other coils: a coat of sealing wax varnish, or of some non-conducting cement, will be necessary.

Since, then, the intensity of the electric current produced in the helix of the electro-magnet, on breaking communication with the battery, increases with the magnetic power of the electro-magnet, with the length and thinness of the wire, with its inverse distance from the iron, and with the regularity of the coiling, the best form of the electro-magnet for the shock, and for decomposition, is that by which the highest

magnetic power can be obtained, and by which the greatest length of thin wire can be coiled on an iron bar, within the smallest distance from the iron, and in a direction at right angles to the axis of the bar.

The magnetic power of an electro-magnet depends, first, on the quantity of iron in the magnet; secondly, on the quantity and intensity of the electricity which flows (in a given time) round the iron bar, and particularly at the poles; thirdly, on the inverse distance between the iron and the electricity flowing round it; fourthly, on the direction in which the electricity flows round the bar; and, fifthly, on the insulation of the wire which carries the voltaic current round the magnet.

First, the magnetic power of an electro-magnet depends on its mass of iron. I have found that small bars are nearly saturated with magnetism, by a single pair of small plates, and that nearly the maximum of magnetic power is given to bars of considerable size by a few large plates. The magnetic power of iron bars saturated with magnetism by the voltaic current is found always to increase with the size of the bars. I believe every one will admit that equal quantities of iron of the same quality have equal capacities for magnetism, and consequently, that the magnetic capacity of an iron bar is proportional to the quantity of iron which it contains. Hence the magnetic power of an electro-magnet cannot be great, unless its mass of iron be considerable.

Secondly, the magnetic power of an electro-magnet depends on the quantity and intensity of the electricity which flows round the magnet in the time necessary for magnetization by the voltaic current. All admit that the magnetic power increases with the quantity of electricity which flows round the bar in a given time: and I have found that it increases very rapidly with the intensity of the electric current, until the iron bar is nearly saturated with magnetism. Many are of opinion that the magnetic power is greatly increased by passing a larger quantity of electricity round the poles, than round any other part of the magnet.

Thirdly, the magnetic power of an electro-magnet depends on the inverse distance between the iron bar, and the electricity flowing round it. If two wires of equal lengths and thickness, be coiled, one over the other, on an iron bar, and if a voltaic current be passed successively through each wire, from the same battery, it will be found that the greatest magnetic power is produced by the current which flows through the wire nearest to the iron. Hence the magnetic power increases with the inverse distance between the iron bar and the electricity flowing round it.

Fourthly, the magnetic power depends on the direction in which the electricity flows round the iron bar. If a voltaic current be sent through a wire coiled on an iron bar, in a direction as nearly as possible, at right angles to the axis of the bar, the magnetic power will be greater than that which can be given to the bar, by an equal current sent through a wire coiled on the bar, in any other direction.

Fifthly, the magnetic power of an electro-magnet depends on the insulation of each coil of wire. If any two coils be in conducting communication with each other, the electric current, instead of flowing round the bar, will pass from one coil to the other in a direction parallel to the axis of the bar.

If the iron bar be straight, the wire can be coiled at right angles to the axis of the bar, with greater facility than when it is of any other form. But a straight bar is unfit for exhibiting magnetic power, and wire can be coiled very regularly on a horse-shoe bar when the curvature is very gradual.

Hence the best method of making an electro-magnet for the shock and for decomposition, is, first, to coil on a long and thick horse-shoe bar of iron of very slow curvature, at right angles to the axis of the bar, once round the whole length, and twice or three times round a few inches at each pole, a very thick copper wire covered with silk or cotton, the ends of which shall be left projecting; secondly, to coil over this wire at right angles to the axis of the magnet, a very long thin wire soldered to the thick one at about twelve inches from one of its extremities; thirdly, to put on each coil of wire a coat of varnish or of non-conducting cement. Such a magnet will have a great capacity for magnetism, a large quantity of electricity of considerable intensity may be transmitted round the iron bar, very near its surface, and at right angles to its axis, and a larger quantity may be sent round the poles than round any other part of the magnet, by connecting the opposite ends of the thick wire with the opposite ends of a voltaic battery containing ten or twenty pairs of large plates. In consequence of the great length and thickness of the iron bar, the remotest coil of the thin wire must be very near the iron. The magnetic power, therefore, of such a magnet must be very great; the long wire coiled round the iron bar is very thin and very near the iron; the insulation of the coils is very perfect; and, consequently, the power of this electro-magnet to produce an electric current of high intensity, must be immense.

If the iron bar be very long and thick, the thick wire coiled once round the whole length of the bar, must be very long, and consequently the quantity of electricity transmitted round

the bar, in the time necessary for magnetization by the voltaic current, will be small compared with the quantity of iron in the magnet. Hence, the magnetic power cannot be very great. The thick wire, therefore, should be divided into two or more equal lengths, and their extremities should be left projecting, so that the voltaic current may be sent through them all, at the same time, and in the same direction. In our large magnet, the iron bar of which is more than 13 feet long, and 2½ inches in diameter, the thick wire is divided into several parts.

The thin wires may be of iron. I have taken shocks from equal lengths of thin iron and copper wires of nearly equal thickness, coiled outside the thick wire, on equal lengths of an electro-magnet. The shock from iron wire well softened appeared to be as strong as the shock from copper wire. The only electro-magnet, from the helix of which I have got the shock, on *making* communication with the battery, is one whose helix consists of about 50 feet of copper wire 1-12th. of an inch thick, and of about 1300 feet of iron wire about 1-40th. of an inch diameter. With a single pair of 7-inch plates, the shock from this magnet is intolerable.

The quantity of electricity which is made to flow through the helix of an electro-magnet, on breaking communication with the battery, depends principally on the magnetic power of the electro-magnet; secondly, on the thickness and shortness of the wire in which the electric current is excited; and thirdly, on its inverse distance from the iron. The most powerful current for ignition, and for the production of magnetism, is obtained from that length and thickness of wire which are required to conduct a voltaic current capable of giving to the enclosed bar the highest magnetic power which the bar can receive from the voltaic current.

In making an electro-magnet, care must be taken to proportion the thickness of the wire intended to conduct the voltaic current by which the iron bar is to be magnetized, to the size of the plates to be employed, and to proportion the length and thickness of the iron bar to the number and size of the plates. For, if the thickness of the wire be less than is required to conduct all the electricity circulated by the plates, there will be a waste of electricity: if it be greater, there will be a waste of wire, and an unnecessary increase of distance between the thin wire and the iron bar. If the iron bar be so large that the voltaic current circulated by the battery will not saturate the bar with magnetism, there will be a waste of iron; and if the bar be so small that the maximum of magnetic power will be given to it by a few pairs of plates, all the other plates in the battery will be useless.

The thickness of wire which will answer best for plates of given size, can be determined only by experiment. I have found that a copper wire 1-10th of an inch diameter will conduct all the electricity circulated by a pair of 7-inch plates. An iron bar 2 feet long and 1¼ inch thick, will be strongly magnetized by a voltaic current passed from a pair of 7-inch plates, through a wire 1-10th of an inch diameter coiled once round the whole length of the bar, and will be nearly saturated with magnetism by an electric current sent through the same wire from 20 pairs of 7-inch plates. I have made a small electro-magnet for the College, which, with a single pair of 7-inch plates gives an exceedingly strong shock. In this magnet, the iron bar is 2 feet long, and 1⅜ inch thick. On the bar, is coiled a copper wire 1-12th of an inch diameter, and about 50 feet long; to the copper wire is soldered a very thin iron wire about 1300 feet long. The voltaic current, by which the iron bar is magnetized, is passed only through the copper wire. The shock is taken by holding in one hand a copper cylinder connected with the beginning of the copper wire, and in the other, a cylinder connected with the end of the iron wire. From the helix of the large electro-magnet which I am now making, I expect to obtain an electric current, equal in point of intensity to that of a battery containing several hundred thousand voltaic circles. The electric current produced by this electro-magnet will probably be capable of decomposing some of those bodies which are now ranked among the simple substances. For the future, batteries containing a large number of plates will be useless; their effects may be produced by a battery of a small number of plates, with the aid of an electro-magnet and of my electro-magnetic repeater.

To any one acquainted with the construction of the common magneto-electric machine, and with all the circumstances on which the electric power of the electro-magnet depends, it must be evident that the electric power which can be obtained by an electro-magnet, is infinitely greater than that of the common magneto-electric machine. The electrical effects produced by the electro-magnet, or by the magneto-electric machine, are proportional to the quantity and intensity of the electricity excited in the magnetic helix, and to the number of electric currents, obtained in a given time. Now, a greater number of electric currents can be obtained, in any time, from the electro-magnet, by means of my electro-magnetic repeater,* than can be produced in the same time, by the magneto-

* Professor Callan's Electro-magnetic Repeater will be found described in our last Number, page 229. Edit.

electric machine. Besides, the electro-magnetic repeater can be worked with far less labour than that which the working of the magneto-electric machine requires.

Secondly, the quantity and intensity of the electricity which may be excited in the helix of the electro-magnet, far exceeds that which can be excited by the magneto-electric machine. First, because it is easy to produce by a voltaic current, a magnetic power infinitely superior to that of the soft iron enclosed by the helix belonging to the magneto-electric machine; secondly, because a far greater length of wire can be coiled on an electro-magnet within a small distance from the iron, than can be coiled on the soft iron surrounded by the helix in the magneto-electric machine. For the electro-magnet may contain a large quantity of iron, but the mass of iron on which the wire is coiled in the magneto-electric machine must be small. By a single pair of zinc plates, a good electro-magnet, and an electro-magnetic repeater, electrical effects can be produced, which far surpass those of the best magneto-electrical machine ever constructed.

LV. *A letter to the Editor of the Annals of Electricity, &c., on a new experiment.*

13, Aberdeen Place,
Maida Hill, April 3d, 1837.

Dear Sir,

The following experiment is, I think, new, and appears to offer matter for speculation; as to the true cause of the phenomenon, at the present time I do not presume to offer any explanation, but am anxious, through the medium of your Annals, to give publicity to the experiment, with a view that others, more accustomed to experiment upon these matters, may examine the subject.

The figure will explain the apparatus employed.

Fig. 81, Plate X., is a slip of sheet zinc cut into the form, and bent, as there represented.

Experiment. When the apparatus for decomposing water, described by Mr. Clarke, in your last number, page 215, is charged with acid and water, or salt and water, and the ends B, B, fig. 81, are made to dip into the mercury of the cups of the decomposing apparatus, fig. 51, Plate VII, the other end *c*, of the zinc being at the same time placed in the vessel containing the platinum wires, the water immediately undergoes decomposition, both platina wires giving out gas, with this singular fact, that the gas evolved from one of the platina

wires is invariably, when compared with the quantity given off by the other, as 2 to 1. The quantity of zinc acted upon need not exceed the 1-16th of an inch.

The volumes of gas in the tubes, would naturally lead to the supposition of one, containing oxygen, and the other hydrogen, but from a series of experiments carefully repeated, I find the gas in both to be hydrogen only, although in every case the proportions were as 2 to 1.

Dear Sir,

Yours faithfully,

GEORGE H. BACHHOFFNER.

To William Sturgeon, Esq., &c.

LVI. *Improvement in the mechanical arrangement of the Hydro-Oxygen Blowpipe.* *By* E. M. Clarke, *Philosophical Instrument Maker.*

Dear Sir,

Permit me through the medium of your valuable and widely circulated Annals to lay before the public a description of my improvement in the hydro-oxygen blowpipe, as described in your last number; to do which, I am obliged to send you a drawing, fig. 1, of that part of Professor Daniell's blowpipe with its two stopcocks A, and B, screwed into the treble connnecting piece C; on the other end of the cocks are two union joints D, and E, to connect them with the pipes which convey the gases to the blowpipe; by this arrangement there are ten separate pieces (requiring seven leather washers to keep them from leakage,) namely A, B, C, three in D, three in E, and the internal tube which screws into A. By my arrangement,

Fig. 1.

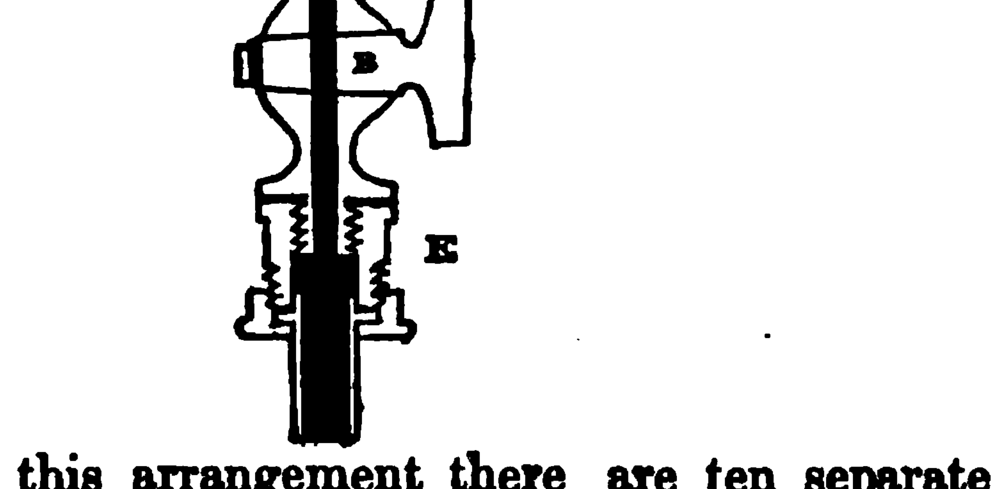


Fig. 2.

fig. 2, the two stopcocks are cast in one piece so as to dispense with the necessity of having the treble connecting piece C, fig. 1, hence I make five separate pieces requiring only two leather washers answer all purposes, namely the piece A, the union joint B, having two pieces C, two pieces equally as well if not better. Part I have left in section to show that the internal tube which conveys the oxygen is soldered inside, so as to remove the possibility of the oxygen leaking into the hydrogen passage, a circumstance neither impossible, nor improbable, in the other arrangement, (fig. 1.) The keys of the stopcocks have the names of the respective gases which they convey, engraved on them. H, H, H″, shows the passage of the hydrogen; O, O′, O″, that of the oxygen. I have added a screw and nut at D, so as to fasten the blowpipe to a clamp stand, fig. 3, by this means you can fix the instrument firmly to any convenient place, for I know by experience that it would be very inconvenient to screw the blowpipe into the gas holder (as represented in the third number of the Annals, fig. 49, Plate VII.) for these reasons, that if the gas holders are placed on the ground, the blowpipe is inconveniently low, and if used at lecture, unless they are before the table would not be seen, if they are elevated on a table or otherwise, they then occupy too much room and are as inconveniently too high for the person who has to keep the pressure up by pouring water into the funnel. I would not recommend using Pepy's gas holder for this purpose at all, as I find that the gasometer is much better adapted for this purpose, (this was first pointed out by my kind and intelligent

Fig. 3.

friend Mr. Cary, whose practical experience on those subjects must be great indeed after producing that splendid instrument which has ranked him the first and best practical optician of our day, namely the hydro-oxygen microscope,) by placing lead weights on top of the bells of the gasometers, you at once obtain a uniform pressure which you never (even with the addition of a ball cock) can obtain from Pepy's gas holder.

Yours, sincerely,
E. M. CLARKE,
Magnetician.

Laboratory of Science,
11, *Lowther Arcade,*
April 23, 1837.

P. S. It may be necessary to explain that as I use the jets and lime holder as originally constructed by Messrs. Cary and Cooper, for their hydro-oxygen microscope, and are already described in your third number, it is superfluous to add figures of them to my present paper.

LVII. *Experimental Researches in Electricity.—Seventh Series. By* MICHAEL FARADAY, *D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution. Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.*

Received January 9,—Read January 23, February 6 and 13, 1834.

§ 11. *On Electro-chemical Decomposition continued.* ¶ iv. *On some general conditions of Electro-decomposition.* ¶ v. *On a new Measurer of Volta-electricity.* ¶ vi. *On the primitive or secondary character of bodies evolved in Electro-decomposition.* ¶ vii. *On the definite nature and extent of Electro-chemical Decompositions.* § 13. *On the absolute quantity of Electricity associated with the particles or atoms of Matter.*

Preliminary.

661. The theory which I believe to be a true expression of the facts of electro-chemical decomposition, and which I have therefore detailed in a former series of these Researches, is so much at variance with those previously advanced, that I find the greatest difficulty in stating results, as I think, correctly, whilst limited to the use of terms which are current with a certain accepted meaning. Of this kind is the term pole, with its prefixes of positive and negative, and the

attached ideas of attraction and repulsion. The general phraseology is that the positive pole *attracts* oxygen, &c., or more cautiously, that it *determines* their evolution upon the surface; and that the negative pole acts in an equal manner upon hydrogen, combustibles, metals, and bases. According to my view, the determining force is *not* at the poles, but *within* the decomposing body; and the oxygen and acids are rendered at the *negative* extremity of that body, whilst hydrogen, metals, &c., are evolved at the *positive* extremity (518. 524.).

662. To avoid, therefore, confusion and circumlocution, and for the sake of greater precision of expression than I can otherwise obtain, I have deliberately considered the subject with two friends, and with their assistance and concurrence in framing them, I purpose henceforward using certain other terms, which I will now define. The poles, as they are usually called, are only the doors or ways by which the electric current passes into and out of the decomposing body (556); and they of course, when in contact with that body, are the limits of its extent in the direction of the current. The term has been generally applied to the metal surfaces in contact with the decomposing substance; but whether philosophers generally would also apply it to the surfaces of air (465. 471.) and water (493.) against which I have effected electro-chemical decomposition, is subject to doubt. In place of the term pole, I propose using that of *Electrode**, and I mean thereby that substance, or rather surface, whether of air, water, metal, or any other body, which bounds the extent of the decomposing matter in the direction of the electric current.

663. The surfaces at which, according to the common phraseology, the electric current enters and leaves a decomposing body, are most important places of action, and require to be distinguished apart from the poles, with which they are mostly, and the electrodes, with which they are always, in contact. Wishing for a natural standard of electric direction to which I might refer these, expressive of their difference and at the same time free from all theory, I have thought it might be found in the earth. If the magnetism of the earth be due to electric currents passing round it, the latter must be in a constant direction, which, according to present usage of speech, would be from east to west, or, which will strengthen this help to the memory, that in which the sun appears to move. If in any case of electro-decomposition we consider the decomposing body as placed so that the current passing

* ἤλεκτρον, and ὁδὸς *a way*.

through it shall be in the same direction, and parallel to that supposed to exist in the earth, then the surfaces at which the electricity is passing into and out of the substance would have an invariable reference, and exhibit constantly the same relations of powers. Upon this notion we purpose calling that towards the east the *anode*,* and that towards the west the *cathode*†; and whatever changes may take place in our views of the nature of electricity and electrical action, as they must affect the natural standard referred to in the same direction, and to an equal amount with any decomposing substances to which these terms may at any time be applied, there seems no reason to expect that they will lead to confusion, or tend in any way to support false views. The *anode* is therefore that surface at which the electric current, according to our present expression, enters: it is the negative extremity of the decomposing body; is where oxygen, chlorine, acids, &c., are evolved; and is against or opposite the positive electrode. The *cathode* is that surface at which the current leaves the decomposing body, and is its positive extremity; the combustible bodies, metals, alkalies, and bases, are evolved there, and it is in contact with the negative electrode.

664. I shall have occasion in these Researches, also, to class bodies together according to certain relations derived from their electrical actions (822.); and wishing to express those relations without at the same time involving the expression of any hypothetical views, I intend using the following names and terms. Many bodies are decomposed directly by the electric current, their elements being set free; these I propose to call *electrolytes*.‡ Water, therefore, is an electrolyte. The bodies which like nitric or sulphuric acids, are decomposed in a secondary manner (752. 757.), are not included under this term. Then for *electro-chemically decomposed*, I shall often use the term *electrolyzed*, derived in the same way, and implying that the body spoken of is separated into its components under the influence of electricity: it is analogous in its sense and sound to *analyze*, which is derived in a similar manner. The term *electrolytical* will be understood at once. Muriatic acid is electrolytical, boracic acid is not.

665. Finally, I require a term to express those bodies which can pass to the *electrodes*, or, as they are usually called, the poles. Substances are frequently spoken of as being *electro-negative*, or *electro-positive*, according as they go under the supposed influence of a direct attraction to the positive or

* *ἀνα upwards, ὁδος a way*; the way which the sun rises.

† *κατα downwards, ὁδος a way*; the way which the sun sets.

‡ ηλεκτρον, and λυω *solvo*. N. Electrolyte, V. Electrolyze.

negative pole. But these terms are much too significant for the use to which I should have to put them; for though the meanings are perhaps right, they are only hypothetical, and may be wrong; and then, through a very imperceptible, but still very dangerous, because continual, influence, they do great injury to science, by contracting and limiting the habitual views of those engaged in pursuing it. I propose to distinguish these bodies by calling those *anions** which go to the *anode* of the decomposing body: and those passing to the *cathode*, *cations*†; and when I have occasion to speak of these together, I shall call them *ions*. Thus the chloride of lead is an *electrolyte*, and when *electrolyzed* evolves the two *ions*, chlorine and lead, the former being an *anion*, and the latter a *cation*.

666. These terms being once well defined, will, I hope, in their use enable me to avoid much periphrasis and ambiguity of expression. I do not mean to press them into service more frequently than will be required, for I am fully aware that names are one thing and science another‡.

667. It will be well understood that I am giving no opinion respecting the nature of the electric current now, beyond what I have done on a former occasion (283. 517.); and that though I speak of the current as proceeding from the parts which are positive to those which are negative (663.), it is merely in accordance with the conventional, though in some degree tacit, agreement entered into by scientific men, that they may have a constant, certain, and definite means of referring to the direction of the forces of that current.

¶ iv. *On some general conditions of Electro-chemical Decomposition.*

669. From the period when electro-chemical decomposition was first effected to the present time, it has been a remark, that those elements which, in the ordinary phenomena of chemical affinity, were the most directly opposed to each other, and combined with the greatest attractive force, were those which were the most readily evolved at the opposite extremities of the decomposing bodies (549.)

670. If this result was evident when water was supposed to be essential to, and was present, in almost every case of

* ἀνιον *that which goes up.* (Neuter participle.)

† κατιον *that which goes down.*

‡ Since this paper was read, I have changed some of the terms which were first proposed, that I might employ only such as were at the same time simple in their nature, clear in their reference, and free from hypothesis.

such decomposition (472.), it is far more evident now that it has been shown and proved that water is not necessarily concerned in the phenomena (474.), and that other bodies much surpass it in some of the effects supposed to be peculiar to that substance.

671. Water, from its constitution and the nature of its elements, and from its frequent presence in cases of electrolytic action, has hitherto stood foremost in this respect. Though a compound formed by very powerful affinity, it yields up its elements under the influence of a very feeble electric current; and it is doubtful whether a case of electrolyzation can occur, where, being present, it is not resolved into its first principles.

672. The various oxides, chlorides, iodides, and salts (402.), which I have shown are decomposable by the electric current when in the liquid state, under the same general law with water, illustrate in an equally striking manner the activity, in such decompositions, of elements directly and powerfully opposed to each other by their chemical relations.

673. On the other hand, bodies dependent on weak affinities, very rarely give way. Take, for instance, glasses: many of those formed of silica, lime, alkali, and oxide of lead, may be considered as little more than solutions of substances one in another*. If bottle-glass be fused, and subjected to the voltaic pile, it does not appear to be at all decomposed (408.). If flint-glass, which contains substances more directly opposed, be operated upon, it suffers some decomposition; and if borate of lead glass, which is a definite chemical compound, be experimented with, it readily yields up its elements (408.).

674. But the result which is found to be so striking in the instances quoted is not at all borne out by reference to other cases where a similar consequence might have been expected. It may be said, that my own theory of electro-chemical decomposition would lead to the expectation that all compound bodies should give way under the influence of the electric current with a facility proportionate to the strength of the affinity by which their elements, either proximate or ultimate, are combined. I am not sure that that follows as a consequence of the theory; but if the objection be supposed one presented by facts, I have no doubt it will be removed when we obtain a more intimate acquaintance with, and precise idea of, the nature of chemical affinity and the mode of action of an electric current over it (518. 524.): besides which, it is just as directly opposed to any other theory of electro-che-

* Philosophical Transactions, 1830, p. 49.

mical decomposition as the one I have propounded; for if it be admitted, as is generally the case, that the more directly bodies are opposed to each other in their attractive forces, the more powerfully do they combine, then the objection applies with equal force to any of the theories of electrolyzation which have been considered, and is an addition to those which I have taken against them.

675. Amongst powerful compounds which are not decomposed, boracic acid stands prominent (408). Then again, the iodide of sulphur, and the chlorides of sulphur, phosphorus, and carbon, are not decomposable under common circumstances, though their elements are of a nature which would lead to a contrary expectation. Chloride of antimony (402. 690), the hydro-carbon, acetic acid, ammonia, and many other bodies undecomposable by the voltaic pile, would seem to be formed by an affinity sufficiently strong to indicate that the elements were so far contrasted in their nature as to sanction the expectation that the pile would separate them, especially as in some cases of mere solution (530. 544.), where the affinity must by comparison be very weak, separation takes place.*

676. It must not be forgotten, however, that much of this difficulty, and perhaps the whole, may depend upon the absence of conducting power, which, preventing the transmission of the current, prevents of course the effects due to it. All known compounds being non-conductors when solid, but conductors when liquid, are decomposed, with *perhaps* the single exception at present known of periodide of mercury (679. 691.); and even water itself, which so easily yields up its elements when the current passes, if rendered quite pure, scarcely suffers change, because it then becomes a very bad conductor.

677. If it should be hereafter proved that the want of decomposition in those cases where, from chemical considerations, it might be so strongly expected (669. 674. 672.), is due to the absence or deficiency of conducting power, it would also be proved, at the same time, that decomposition *depends* upon conduction, and not the latter upon the former (413.); and in water this seems to be very nearly decided. On the other hand, the conclusion is almost irresistible, that in electrolytes the power of transmitting the electricity across the substance is dependent upon their capability of suffering

* With regard to solution, I have met with some reasons for supposing that it will probably disappear as a cause of transference, and intend resuming the consideration at a convenient opportunity.

decomposition; taking place only whilst they are decomposing, and being proportionate to the quantity of elements separated (821.). I may not, however, stop to discuss this point experimentally at present.

678. When a compound contains such elements as are known to pass towards the opposite extremities of the voltaic pile, still the proportions in which they are present appear to be intimately connected with capability in the compound of suffering or resisting decomposition. Thus, the protochloride of tin readily conducts, and is decomposed (402.), but the perchloride neither conducts nor is decomposed (406.). The protiodide of tin is decomposed when fluid (402.); the periodide is not (405.). The periodide of mercury when fused is not decomposed (691.), even though it does conduct. I was unable to contrast it with the protiodide, the latter being converted into mercury and periodide by heat.

679. These important differences induced me to look more closely to certain binary compounds, with the view of ascertaining whether a *law* regulating the *decomposability* according to some *relation of the proportionals or equivalents* of the elements, could be discovered. The proto compounds only, amongst those just referred to, were decomposable; and on referring to the substances quoted to illustrate the force and generality of the law of conduction and decomposition which I discovered (402.), it will be found that all the oxides, chlorides, and iodides subject to it, except the chloride of antimony and the periodide of mercury, (to which may now perhaps be added corrosive sublimate), are also decomposable, whilst many per compounds of the same elements, not subject to the law, were not so (405. 406.).

680. The substances which appeared to form the strongest exceptions to this general result were such bodies as the sulphuric, phosphoric, nitric, arsenic, and other acids.

681. On experimenting with sulphuric acid, I found no reason to believe that it was by itself a conductor of, or decomposable by, electricity, although I had previously been of that opinion (552.). When very strong it is a much worse conductor than if diluted*. If then subjected to the action of a powerful battery, oxygen appears at the *anode*, or positive electrode, although much is absorbed (728.), and hydrogen and sulphur appear at the *cathode*, or negative electrode. Now the hydrogen has with me always been pure, not sulphuretted, and has been deficient in proportion to the sulphur present, so that it is evident that when decomposition

* De la Rive.

occurred, water must have been decomposed. I endeavoured to make the experiment with anhydrous sulphuric acid. It appeared to me that in that state, when fused, sulphuric acid was not a conductor, nor decomposed; but I had not enough of the dry acid in my possession to allow me to decide the point satisfactorily. My belief is, that when sulphur appears by the action of the pile on sulphuric acid, it is the result of a secondary action, and that the acid itself is not electrolyzable (757.).

682. Phosphoric acid is, I believe, also in the same condition; but I have found it impossible to decide the point, because of the difficulty of operating on fused anhydrous phosphoric acid. Phosphoric acid which has once obtained water cannot be deprived of it by heat alone. When heated, the hydrated acid volatilizes. Upon subjecting phosphoric acid, fused upon the ring end of a wire (401.), to the action of the voltaic apparatus, it conducted, and was decomposed; but gas, which I believe to be hydrogen, was always evolved at the negative electrode, and the wire was not affected, as would have happened had phosphorus been separated. Gas was also evolved at the positive electrode. From all the facts, I conclude it was the water and not the acid which was decomposed.

683. *Arsenic acid.* This substance conducted, and was decomposed; but it contained water, and I was unable at the time to press the investigation so as to ascertain whether a fusible anhydrous arsenic acid could be obtained. It forms, therefore, at present no exception to the general result.

684. Nitrous acid, obtained by distilling nitrate of lead, and keeping it in contact with strong sulphuric acid, was found to conduct and decompose slowly. But on examination there were strong reasons for believing that water was present, and that the decomposition and conduction depended upon it. I endeavoured to prepare a perfectly anhydrous portion, but could not spare the time required to procure an unexceptionable result.

685. Nitric acid is a substance which I believe is not decomposed directly by the electric current. As I want the facts in illustration of the distinction existing between primary and secondary decomposition, I will merely refer to them in this place (752.).

686. That these mineral acids should confer facility of conduction and decomposition on water, is no proof that they are competent to favour and suffer these actions in themselves. Boracic acid does the same thing, though not decomposable. M. De la Rive has pointed out that chlorine has this power

also; but being to us an elementary substance, it cannot be due to its capability of suffering decomposition.

687. *Chloride of sulphur* does not conduct, nor is it decomposed. It consists of single proportionals of its elements, but is not on that account an exception to the rule (679.), which does not affirm that *all* compounds of single proportionals of elements are decomposable, but that such as are decomposable are so constituted.

688. *Protochloride of phosphorus* does not conduct nor become decomposed.

689. *Protochloride of carbon* does not conduct nor suffer decomposition. In association with this substance, I submitted the *hydro-chloride of carbon* from olefiant gas and chlorine to the action of the electric current; but it also refused to conduct or yield up its elements.

690. With regard to the exceptions (679.), upon closer examination, some of them disappear. Chloride of antimony (a compound of one proportional of antimony and one and a half of chlorine) of recent preparation was put into a tube (fig. 82, Plate XI.) (789.), and submitted when fused to the action of the current, the positive electrode being of plumbago. No electricity passed, and no appearance of decomposition was visible at first; but when the positive and negative electrodes were brought very near each other in the chloride, then a feeble action occurred and a feeble current passed. The effect altogether was so small (although quite amenable to the law before given), and so unlike the decomposition and conduction occurring in all the other cases, that I attribute it to the presence of a minute quantity of water, (for which this and many other chlorides have strong attractions, producing hydrated chlorides,) or perhaps of a true protochloride consisting of single proportionals (695. 796.).

691. *Periodide of mercury* being examined in the same manner, was found most distinctly to insulate whilst solid, but conduct when fluid, according to the law of *liquido-conduction* (402.); but there was no appearance of decomposition. No iodine appeared at the *anode*, nor mercury or other substance at the *cathode*. The case is, therefore, no exception to the rule, that only compounds of single proportionals are decomposable; but it is an exception, and I think the only one, to the statement, that all bodies subject to the law of liquido-conduction are decomposable. I incline, however, to believe, that a portion of protiodide of mercury is retained dissolved in the periodide, and that to its slow decomposition the feeble conducting power is due. Periodide would be formed, as a secondary result, at the *anode*; and the mer-

cury at the *cathode* would also form, as a secondary result, protiodide Both these bodies would mingle with the fluid mass, and thus no final separation appear, notwithstanding the continued decomposition.

692. When *perchloride of mercury* was subjected to the voltaic current, it did not conduct in the solid state, but it did conduct when fluid. I think, also, that in the latter case it was decomposed; but there are many interfering circumstances which require examination before a positive conclusion can be drawn.

693. When the ordinary protoxide of antimony is subjected to the voltaic current in a fused state, it also is decomposed, although the effect from other causes soon ceases (402. 802.). This oxide consists of one proportional of antimony and one and a half of oxygen, and is therefore an exception to the general law assumed. But in working with this oxide and the chloride, I observed facts which lead me to doubt whether the compounds usually called the protoxide and the protochloride do not often contain other compounds, consisting of single proportions, which are the true proto compounds, and which, in the case of the oxide, might give rise to the decomposition above described.

694. The ordinary sulphuret of antimony is considered as being the compound with the smallest quantity of sulphur, and analogous in its proportions to the ordinary protoxide. But I find that if it be fused with metallic antimony, a new sulphuret is formed, containing much more of the metal than the former, and separating distinctly, when fused, both from the pure metal on the one hand, and the ordinary grey sulphuret on the other. In some rough experiments, the metal thus taken up by the ordinary sulphuret of antimony was equal to half the proportion of that previously in the sulphuret, in which case the new sulphuret would consist of *single* proportionals.

695. When this new sulphuret was dissolved in muriatic acid, although a little antimony separated, yet it appeared to me that a true protochloride, consisting of *single* proportionals, was formed, and from that, by alkalies, &c., a true protoxide, consisting also of *single* proportionals was obtainable. But I could not stop to ascertain this matter strictly by analysis.

696. I believe, however, that there is such an oxide; that it is often present in variable proportions in what is commonly called protoxide, throwing uncertainty upon the results of its analysis, and causing the electrolytic decomposition above described.

697. Upon the whole, it appears probable that all those binary compounds of elementary bodies which are capable of being electrolyzed when fluid, but not whilst solid, according to the law of liquido-conduction (394.), consist of single proportionals of their elementary principles; and it may be because of their departure from this simplicity of composition, that boracic acid, ammonia, perchlorides, periodides, and many other direct compounds of elements, are indecomposable.

698. With regard to salts and combinations of compound bodies, the same simple relation does not appear to hold good. I could not decide this by bisulphates of the alkalies, for as long as the second proportion of acid remained, water was retained with it. The fused salt, therefore, conducted, and was decomposed; but hydrogen always appeared at the negative electrode.

699. A biphosphate of soda was prepared by heating, and ultimately fusing, the ammoniate-phosphate of soda. In this case the fused bisalt conducted, and was decomposed; but a little gas appeared at the negative electrode, and though I believe the salt itself was electrolyzed, I am not quite satisfied that water was entirely absent.

700. Then a biborate of soda was prepared; and this I think, is an unobjectionable case. The salt, when fused, conducted, and was decomposed, and gas appeared at both electrodes: even when the boracic acid was increased in three proportionals the same effect took place.

701. Hence this class of compound combinations does not seem to be subject to the same simple law as the former class of binary combinations. Whether we may find reason to consider them as mere solutions of the compound of single proportionals in the excess of acid, is a matter which, with some apparent exceptions occurring amongst the sulphurets, must be left for decision by future examination.

702. In any investigation of these points, great care must be taken to exclude water; for if present, secondary effects are so frequently produced as often seemingly to indicate an electro-decomposition of substances, when no true result of the kind has occurred (742. &c.).

703. It is evident that all the cases in which decomposition *does not occur may* depend upon the want of conduction (677. 413.); but that does not at all lessen the interest excited by seeing the great difference of effect due to a change, not in the nature of the elements, but merely in their proportions, especially in any attempt which may be made to elucidate and expound the beautiful theory put forth by Sir Humphry

DAVY*, and illustrated by BERZELIUS and other eminent philosophers, that ordinary chemical affinity is a mere result of the electrical attraction of the particles of matter.

¶ v. *On a new measurer of Volta-electricity.*

704. I have already said, when engaged in reducing common and voltaic electricity to one standard of measurement (377.), and again when introducing my theory of electro-chemical decomposition (504. 505. 510.), that the chemical decomposing action of a current *is constant for a constant quantity of electricity*, notwithstanding the greatest variations in its sources, in its intensity, in the size of the *electrodes* used, in the nature of the conductors (or non-conductors (307.) through which it is passed, or in other circumstances. The conclusive proofs of the truth of these statements shall be given almost immediately (783, &c.).

705. I endeavoured upon this law to construct an instrument which should measure out the electricity passing through it, and which, being interposed in the course of the current used in any particular experiment, should serve at pleasure, either as a *comparative standard* of effect, or as a *positive measurer* of this subtile agent.

706. There is no substance better fitted, under ordinary circumstances, to be the indicating body in such an instrument than water; for it is decomposed with facility when rendered a better conductor by the addition of acids or salts; its elements may in numerous cases be obtained and collected without any embarrassment from secondary action, and, being gaseous, they are in the best physical condition for separation and measurement. Water, therefore, acidulated by sulphuric acid, is the substance I shall generally refer to, although it may become expedient in peculiar cases or forms of experiment to use other bodies (843.).

707. The first precaution needful in the construction of the instrument was to avoid the recombination of the evolved gases, an effect which the positive electrode has been found so capable of producing (571.). For this purpose various forms of decomposing apparatus were used. The first consisted of straight tubes, each containing a plate and wire of platina soldered together by gold, and fixed hermetically in the glass at the closed extremity of the tube (Plate XI. fig. 83.). The tubes were about eight inches long, 0·7 of an inch in diameter, and graduated. The platina plates were about an inch long,

* Philosophical Transactions, 1807, pp. 32, 39; also 1826, pp. 387, 389.

as wide as the tubes would permit, and adjusted as near to the mouths of the tubes as was consistent with the safe collection of the gases evolved. In certain cases, where it was required to evolve the elements upon as small a surface as possible, the metallic extremity, instead of being a plate, consisted of the wire bent into the form of a ring (fig. 84.). When these tubes were used as measurers, they were filled with the dilute sulphuric acid, and inverted in a basin of the same liquid (fig. 85.), being placed in an inclined position, with their mouths near to each other, that as little decomposing matter should intervene as possible; and also, in such a direction that the platina plates should be in vertical planes (720.).

708. Another form of apparatus was that delineated (fig. 86). The tube is bent in the middle; one end is closed; in that end is fixed a wire and plate, *a*, proceeding so far downwards, that, when in the position figured, it shall be as near to the angle as possible, consistently with the collection, at the closed extremity of the tube, of all the gas evolved against it. The plane of this plate is also perpendicular (720.). The other metallic termination, *b*, is introduced at the time decomposition is to be effected, being brought as near the angle as possible, without causing any gas to pass from it towards the closed end of the instrument. The gas evolved against it is allowed to escape.

709. The third form of apparatus contains both electrodes in the same tube; the transmission, therefore, of the electricity, and the consequent decomposition, is far more rapid than in the separate tubes. The resulting gas is the sum of the portions evolved at the two electrodes, and the instrument is better adapted than either of the former as a measurer of the quantity of voltaic electricity transmitted in ordinary cases. It consists of a straight tube (fig. 87.) closed at the upper extremity, and graduated, through the sides of which pass the platina wires (being fused into the glass), which are connected with two plates within. The tube is fitted by grinding into one mouth of a double necked bottle. If the latter be one half or two thirds full of the dilute sulphuric acid, it will, upon upon inclination of the whole, flow into the tube and fill it. When an electric current is passed through the instrument, the gases evolved against the plates collect in the upper portion of the tube, and are not subject to the recombining power of the platina.

710. Another form of the instrument is given at fig. 88.

711. A fifth form is delineated (fig. 89.) This I have found exceedingly useful in experiments continued in succession for days together, and where large quantities of indicating gas

were to be collected. It is fixed on a weighted foot, and has the form of a small retort containing the two electrodes: the neck is narrow, and sufficiently long to deliver gas issuing from it into a jar placed in a small pneumatic trough. The electrode chamber, sealed hermetically at the part held in the stand, is five inches in length, and 0·6 of an inch in diameter; the neck about nine inches in length, and 0·4 of an inch in diameter internally. The figure will fully indicate the construction.

712. It can hardly be requisite to remark, that in the arrangement of any of these forms of apparatus, they, and the wires connecting them with the substance, which is collaterally subjected to the action of the same electric current, should be so far insulated as to ensure a certainty that all the electricity which passes through the one shall also be transmitted through the other.

713. Next to the precaution of collecting the gases, if mingled, out of contact with the platinum, was the necessity of testing the law of a *definite electrolytic* action, upon water at least, under all varieties of condition; that, with a conviction of its certainty, might also be obtained a knowledge of those interfering circumstances which would require to be practically guarded against.

714. The first point investigated was the influence or indifference of extensive variations in the size of the electrodes, for which purpose instruments like those last described (709. 710. 711.) were used. One of these had plates 0·7 of an inch wide, and nearly four inches long; another had plates 0·5 of an inch wide, and 0·8 of an inch long; a third had wires 0·02 of an inch in diameter, and three inches long: and a fourth similar wires only half an inch in length. Yet when these were filled with dilute sulphuric acid, and, being placed in succession, had one common current of electricity passed through them, very nearly the same quantity of gas was evolved in all. The difference was sometimes in favour of one, and sometimes on the side of another; but the general result was that the largest quantity of gases was evolved upon the smaller surface of the wires.

715. Experiments of a similar kind were made with the single-plate, straight tubes (707.), and also with the curved tubes (708.), with similar consequences; and when these, with the former tubes, were arranged together in various ways, the result, as to the equality of action of large and small metallic surfaces when delivering and receiving the same current of electricity, was constantly the same. As an illustration, the following numbers are given. An instrument

with two wires evolved 74·3 volumes of mixed gases; another with plates 73·25 volumes; whilst the sum of the oxygen and hydrogen in two separate tubes amounted to 73·65 volumes. In an other experiment the volumes were 55·3, 55·3, and 54·4.

716. But it was observed in these experiments, that in single-plate tubes (707.) more hydrogen was evolved at the negative electrode than was proportionate to the oxygen at the positive electrode; and generally, also, more than was proportionate to the oxygen and hydrogen in a double-plate tube. Upon more minutely examining these effects, I was led to refer them, and also the differences between wires and plates (714.), to the solubility of the gases evolved, especially at the positive electrode.

717. When the positive and negative electrodes are equal in surface, the bubbles which rise from them in dilute sulphuric acid are always different in character. Those from the positive plate are exceedingly small, and separate instantly from every part of the surface of the metal, in consequence of its perfect cleanliness (633.); whilst in the liquid they give it a hazy appearance, from their number and minuteness; are easily carried down by currents; and therefore not only present far greater surface of contact with the liquid than larger bubbles would do, but are retained a much longer time in mixture with it. But the bubbles at the negative surface, though they constitute twice the volume of the gas at the positive electrode, are nevertheless very inferior in number. They do not rise so universally from every part of the surface, but seem to be evolved at different points: and though so much larger, they appear to cling to the metal, separating with difficulty from it, and when separated, instantly rising to the top of the liquid. If, therefore, oxygen and hydrogen had equal solubility in, or powers of combining with, water under similar circumstances, still under the present conditions the oxygen would be far the most liable to solution; but when to these is added its well known power of forming a compound with water, it is no longer surprising that such a compound should be produced in small quantities at the positive electrode; and indeed the bleaching power which some philosophers have observed in a solution at this electrode, when chlorine and similar bodies have been carefully excluded, is probably due to the formation there, in this manner, of oxy-water.

718. That more gas was collected from the wires than from the plates, I attribute to the circumstance, that as equal quantities were evolved in equal times, the bubbles at the wires having been more rapidly produced, in relation to any part of

the surface, must have been much larger: have been therefore in contact with the fluid by a much smaller surface, and for a much shorter time than those at the plates: hence less solution and a greater collection.

719. There was also another effect produced, especially by the use of large electrodes, which was both a consequence and a proof of the solution of part of the gas evolved there. The collected gas, when examined, was found to contain small portions of nitrogen. This I attribute to the presence of air dissolved in the acid used for decomposition. It is a well-known fact, that when bubbles of a gas but slightly soluble in water or solutions pass through them, the portion of this gas which is dissolved displaces a portion of that previously in union with the liquid: and so, in the decompositions under consideration, as the oxygen dissolves, it displaces a part of the air, or at least of the nitrogen, previously united to the acid; and this proceeds *most extensively* with large plates, because the gas evolved at them is in the most favourable condition for solution.

720. With the intention of avoiding this solubility of the gases as much as possible, I arranged the decomposing plates in a vertical position (707. 708.), that the bubbles might quickly escape upwards, and that the downward currents in the fluid should not meet ascending currents of gas. This precaution I found to assist greatly in producing constant results, and especially in experiments to be hereafter referred to, in which other liquids than dilute sulphuric acid, as for instance solution of potash, were used.

721. The irregularities in the indications of the measurer proposed, arising from the solubility just referred to, are but small, and may be very nearly corrected by comparing the results of two or three experiments. They may also be almost entirely avoided by selecting that solution which is found to favour them in the least degree (728.); and still further by collecting the hydrogen only, and using that as the indicating gas; for being much less soluble than oxygen, being evolved with twice the rapidity and in larger bubbles (717.), it can be collected more perfectly and in greater purity.

722. From the foregoing and many other experiments, it results that *variation in the size of the electrodes causes no variation in the chemical action of a given quantity of electricity upon water.*

723. The next point in regard to which the principle of constant electro-chemical action was tested, was *variation of intensity.* In the first place, the preceding experiments were repeated, using batteries of an *equal* number of plates,

strongly and *weakly* charged; but the results were alike. They were then repeated, using batteries sometimes containing forty, and at other times only five pairs of plates: but the results were still the same. *Variations therefore in the intensity*, caused by difference in the strength of charge, or in the number of alternations used, *produced no difference as to the equal action of large and small electrodes.*

724. Still these results did not prove that variation in the intensity of the current was not accompanied by a corresponding variation in the electro-chemical effects, since the actions at *all* the surfaces might have increased or diminished together. The deficiency in the evidence is, however, completely supplied by the former experiments on different-sized electrodes: for with variation in the size of these, a variation in the intensity must have occurred. The intensity of an electric current traversing conductors alike in their nature, quality, and length, is probably as the quantity of electricity passing through a given sectional area perpendicular to the current, divided by the time (360. *note*); and therefore when large plates were contrasted with wires separated by an equal length of the same decomposing conductor (714.), whilst one current of electricity passed through both arrangements, that electricity must have been in a very different state, as to *tension*, between the plates and between the wires; yet the chemical results were the same,

725. The difference in intensity, under the circumstances described, may be easily shown practically, by arranging two decomposing apparatus as in fig. 90, where the same fluid is subjected to the decomposing power of the same current of electricity, passing in the repel A. between large platina plates, and in the vessel B. between small wires. If a third decomposing apparâtus, such as that delineated fig. 89. (711.), be connected with the wires at *a b*, fig. 90. it will serve sufficiently well, by the degree of decomposition occurring in it, to indicate the relative state of the two plates as to intensity; and if it then be applied in the same way, as a test of the state of the wires at *a′ b′*, it will, by the increase of decomposition within, show how much greater the intensity is there than at the former points. The connexions of P and N with the voltaic battery are of course to be continued during the whole time.

726. A third form of experiment in which difference of intensity was obtained, for the purpose of testing the principle of equal chemical action, was to arrange three volta-electrometers, so that after the electric current had passed through one, it should divide into two parts, which, after traversing

each one of the remaining instruments, should reunite. The sum of the decomposition in the two latter vessels was always equal to the decomposition in the former vessel. But the *intensity* of the divided current could not be the same as that it had in its original state; and therefore *variation of intensity has no influence on the results if the quantity of electricity remain the same.* The experiment, in fact, resolves itself simply into an increase in the size of the electrodes (725.).

727. The *third point*, in respect to which the principle of equal electro-chemical action on water was tested, was *variation of the strength of the solution used.* In order to render the water a conductor, sulphuric acid had been added to it (707.); and it did not seem unlikely that this substance, with many others, might render the water more subject to decomposition, the electricity remaining the same in quantity. But such did not prove to be the case. Diluted sulphuric acid, of different strengths, was introduced into different decomposing apparatus, and submitted simultaneously to the action of the same electric current (714.). Slight differences occurred, as before, sometimes in one direction, sometimes in another; but the final result was, that *exactly the same quantity of water was decomposed in all the solutions by the same quantity of electricity,* though the sulphuric acid in some was seventy-fold what it was in others. The strength used were of specific gravity 1.495, and downwards.

728. When an acid having a specific gravity of about 1·336 was employed, the results were most uniform, and the oxygen and hydrogen (716.) most constantly in the right proportion to each other. Such an acid gave more gas than one much weaker acted upon by the same current, apparently because it had less solvent power. If the acid were very strong, then a remarkable disappearance of oxygen took place; thus, one made by mixing two measures of strong oil of vitriol with one of water, gave forty-two volumes of hydrogen, but only twelve of oxygen. The hydrogen was very nearly the same with that evolved from acid of the specific gravity 1·232. I have not yet had time to examine minutely the circumstances attending the disappearance of the oxygen in this case, but imagine it is due to the formation of oxywater, which THENARD has shown is favoured by the presence of acid.

729. Although not necessary for the practical use of the instrument I am describing, yet as connected with the important point of constant electro-chemical action upon water, I now investigated the effects produced by an electric current passing through aqueous solutions of acids, salts, and compounds, exceedingly different from each other in their nature,

and found them to yield astonishingly uniform results. But many of them which are connected with a secondary action will be more usefully described hereafter (778.).

730. When solutions of caustic potassa or soda, or sulphate of magnesia, or sulphate of soda, were acted upon by the electric current, just as much oxygen and hydrogen was evolved from them as from the diluted sulphuric acid, with which they were compared. When a solution of ammonia, rendered a better conductor by sulphate of ammonia (554.), or a solution of subcarbonate of potassa was experimented with, the *hydrogen* evolved was in the same quantity as that set free from the diluted sulphuric acid with which they were compared. Hence *changes in the nature of the solution do not alter the constancy of electrolytic action upon water.*

731. I have already said, respecting large and small electrodes, that change of order caused no change in the general effect (715.). The same was the case with different solutions, or with different intensities; and however the circumstances of an experiment might be varied, the results came forth exceedingly consistent, and proved that the electro-chemical action was still the same.

732. I consider the foregoing investigation as sufficient to prove the very extraordinary and important principle with respect to WATER, *that when subjected to the influence of the electric current, a quantity of it is decomposed exactly proportionate to the quantity of electricity which has passed,* notwithstanding the thousand variations in the conditions and circumstances under which it may at the time be placed; and further, that when the interference of certain secondary effects (742. &c.), together with the solution or recombination of the gas and the evolution of air, are guarded against, *the products of the decomposition may be collected with such accuracy, as to afford a very excellent and valuable measurer of the electricity concerned in their evolution.*

733. The forms of instrument which I have given, figs. 87, 88, 89. (709. 710. 711.), are probably those which will be found most useful, as they indicate the quantity of electricity by the largest volume of gases, and cause the least obstruction to the passage of the current. The fluid which my present experience leads me to prefer, is a solution of sulphuric acid of specific gravity about 1·336, or from that to specific gravity 1·25; but it is very essential that there should be no organic substance, nor any vegetable acid, nor other body, which, by being liable to the action of the oxygen or hydrogen evolved at the electrodes (773. &c.), shall diminish their quantity, or add other gases to them.

734. In many cases when the instrument is used as a *comparative standard,* or even as *a measurer,* it may be desirable to collect the hydrogen only, as being less liable to absorption or disappearance in other ways than the oxygen; whilst at the same time its volume is so large, as to render it a good and sensible indicator. In such cases the first and second form of apparatus have been used, figg. 85. 86. (707. 708.) The indications obtained were very constant, the variations being much smaller than in those forms of apparatus collecting both gases; and they can also be procured when solutions are used in comparative experiments, which, yielding no oxygen or only secondary results of its action, can give no indications if the educts at both electrodes be collected. Such is the case when solutions of ammonia, muriatic acid, chlorides, iodides, acetates, or other vegetable salts, &c., are employed.

735. In a few cases, as where solutions of metallic salts liable to reduction at the negative electrode are acted upon, the oxygen may be advantageously used as the measuring substance. This is the case, for instance, with sulphate of copper.

736. There are therefore two general forms of the instrument which I submit as a measurer of electricity. One, in which both the gases of the water decomposed are collected (709. 710. 711.): and the other, in which a single gas, as the hydrogen only, is used (707. 708.). When referred to as a *comparative instrument,* (a use I shall now make of it very extensively,) it will not often require particular precaution in the observation; but when used as an *absolute measurer,* it will be needful that the barometric pressure and the temperature be taken into account, and that the graduation of the instruments should be to one scale; the hundredths and smaller divisions of a cubical inch are quite fit for this purpose, and the hundredth may be very conveniently taken as indicating a DEGREE of electricity.

737. It can scarcely be needful to point out further than has been done how this instrument is to be used. It is to be introduced into the course of the electric current, the action of which is to be exerted anywhere else, and if 60° or 70° of electricity are to be measured out, either in one or several portions, the current, whether strong or weak, is to be continued until the gas in the tube occupies that number of divisions or hundredths of a cubical inch. Or if a quantity competent to produce a certain effect is to be measured, the effect is to be obtained, and then the indication read off. In exact experiments it is necessary to correct the volume of gas for changes

in temperature and pressure, and especially for moisture.* For the latter object the volta-electrometer (fig. 89.) is most accurate, as its gas can be measured over water, whilst the others retain it over acid or saline solutions.

738. I have not hesitated to apply the term *degree,* in analogy with the use made of it with respect to another most important imponderable agent, namely, heat; and as the definite expansion of air, water, mercury, &c., is there made use of to measure heat, so the equally definite evolution of gases is here turned to a similar use for electricity.

739. The instrument offers the only *actual measurer* of voltaic electricity which we at present possess. For without being at all affected by variations in time on intensity, or alterations in the current itself, of any kind, or from any cause, or even of intermissions of action, it takes note with accuracy of the quantity of electricity which has passed through it, and reveals that quantity by inspection; I have therefore named it a VOLTA-ELECTROMETER.

740. Another mode of measuring volta-electricity may be adopted with advantage in many cases, dependent on the quantities of metals or other substances evolved either as primary or as secondary results; but I refrain from enlarging on this use of the products, until the principles on which their constancy depends have been fully established (791. 843.).

741. By the aid of this instrument I have been able to establish the definite character of electro-chemical action in its most general sense; and I am persuaded it will become of the utmost use in the extensions of the science which these views afford. I do not pretend to have made its detail perfect, but to have demonstrated the truth of the principle, and the utility of the application.

¶ vi. *On the primary or secondary character of the bodies evolved at the Electrodes.*

742. Before the *volta-electrometer* could be employed in determining, as a *general law*, the constancy of electro-decomposition, it became necessary to examine a distinction, already recognised among scientific men, relative to the products of that action, namely, their primitive or secondary character; and, if possible, by some general rule or principle, to decide when they were of the one or the other kind. It will appear hereafter that great mistakes respecting electro-chemical

* For a simple table of correction for moisture, I may take the liberty of referring to my Chemical Manipulation, edition of 1830, p. 376.

action and its consequences, have arisen from confounding these two classes of results together.

743. When a substance under decomposition yields at the electrodes those bodies uncombined and unaltered which the electric current has separated, then they may be considered as primary results, even though themselves compounds. Thus the oxygen and hydrogen from water are primary results; and so also are the acid and alkali (themselves compound bodies) evolved from sulphate of soda. But when the substances separated by the current are changed at the electrodes before their appearance, then they give rise to secondary results, although in many cases the bodies evolved are elementary.

744. These secondary results occur in two ways, being sometimes due to the mutual action of the evolving substance and the matter of the electrode, and sometimes to its action upon the substances contained in the decomposing conductor itself. Thus, when carbon is made the positive electrode in dilute sulphuric acid, carbonic oxide and carbonic acid appear there instead of oxygen; for the latter, acting upon the matter of the electrode, produces these secondary results. Or if the positive electrode, in a solution of nitrate or acetate of lead, be platina, then peroxide of lead appears there, equally a secondary result with the former, but now depending upon an action of the oxygen on a substance in the solution. Again, when ammonia is decomposed by platina electrodes, nitrogen appears at the *anode**; but though an *elementary* body, it is a *secondary* result in this case, being derived from the chemical action of the oxygen electrically evolved there, upon the ammonia in the surrounding solution (554). In the same manner when aqueous solutions of metallic salts are decomposed by the current, the metals evolved at the *cathode*, though elements, are *always* secondary results, and not immediate consequences of the decomposing power of the electric current.

745. Many of these secondary results are extremely valuable; for instance, all the interesting compounds which M. Becquerel has obtained by feeble electric currents are of this nature; but they are essentially chemical, and must, in the theory of electrolytic action, be carefully distinguished from those which are directly due to the action of the electric current.

To be continued.

* Annales de Chimie, 1804, tom. li. p. 167.

LVIII. MISCELLANEOUS ARTICLES.

Extract from a letter to Mr. Sturgeon.

Sir,

I am a subscriber to your excellent periodical, for which I take the present opportunity of rendering you my thanks, as I find great instruction from several of the valuable articles it contains. I have for some time been experimenting in Electro-magnetism and Magnetic-electricity, and am at present engaged in fitting up a magnetic-electrical machine; for, although an amateur mechanician, I make several of my own apparatus, and am now desirous of having one of these machines of my own construction. As, however, I am still in want of information on some particular points, and I suppose many of your readers are equally so, I should be glad to see the following subjects treated on in an early number of your work. In what manner is a jar to be charged, and the gold leaves of an electroscope to be deflected by a magnetic-electrical machine?

It is stated in your first number that Mr. Crosse has a "round conductor for a minimum of power, instead of a combination of flat or parallel ones for a maximum." Does this imply that a compound prime conductor formed of a number of thin, flat, and parallel plates will strike a more powerful spark than the cylindrical form usually employed? I have always supposed the cylindric form with rounded ends the best. Mr. Brookes found that a combination of small cylinders arranged like the bars of a gridiron not near so good. I hope that some of your correspondents will treat scientifically on this subject in an early number of your Annals, as I am sure it will be interesting to many of your readers.

I will now mention a little improvement which occurred to me some time since in making an electro-magnet. These magnets are usually made (as at the Adelaide Gallery) by soldering all the ends of the several wires, at each end of the compound helix, to a thick piece of wire, which, of course, becomes thus permanently fixed. Thus the magnet has always two long clumsy dangling wires fixed to it: which, in point of portability, is very awkward. I have therefore fixed two little cups of copper tube, one on each branch of the magnet. In these cups are placed portions of mercury, in which are immersed all the extremities of the coil wires.

The thick copper wires then are separate, and bent at the ends into a right angle, so that they may fit into the cups both at the magnet and at the battery. The lower ends of the cups at the magnet have slips of copper, by which they are bound down by the green silk ribband wound over the whole when finished, which gives to the apparatus a very neat appearance. This plan is equally good in effect, and incomparably better in convenience, as the magnet can now be put away into a small drawer or box, which otherwise would not contain it by some feet. This plan may possibly have occurred to others, but if so I have never heard of it.

I am, Sir,
Your most obedient,
J.

Exeter, Feb. 16. 1837.

Aurora Borealis, of April 22, 1836, seen by J. Mc. Caffrey, *at St. Mary's College, Emmitsburgh.*

"We had the good fortune to witness, on the night of the 22d, and morning of the 23d, April, a most magnificent display of the Northern Lights. This phenomenon is but rarely seen in our latitude, and more rarely still, appears to us in its highest splendour. As accurate a description, therefore, as may be given of its late remarkable appearance, with a statement of such facts, as may tend in any degree to elucidate the questions which it presents, cannot be unacceptable, and may prove useful. I begin by noting the state of the weather at the time: although I am not aware that the Aurora either influences it, or is affected by it. The day was fair during the whole of the 22d. A high wind blew from the north-west until mid-day, when it became calm. At nightfall, the wind again arose, and increased gradually so as to be moderately high by two o'Clock the next morning. At dusk it had shifted to the west. The temperature was 39° at sunrise of the 22d, and 53° at three o'Clock P. M. From ten o'Clock that night until two the following morning, the thermometer stood at 38°; the barometer remained stationary at 30·00 throughout the evening and night.

"About seven o'Clock of the evening of the 22d, it was noticed that a large part of the northern heavens was covered with a thin vapour-like appearance—white at the base—of a pale red at the upper edges, and of a deeper hue, red and yellow intermixed, about the middle. It spread through an arc of 60° near the horizon, and extended halfway up to the zenith.

Before nine o'Clock it had disappeared, leaving nothing but a bank of white auroral vapour, stretching along the north and north-east horizon. At fifteen minutes after ten, on looking towards the north, I perceived a few well-defined columns, shooting up a short distance, each of them appearing and vanishing momentarily; yet so that to a careless observer, they might seem to remain permanently before the view. Some of my friends were now called up to enjoy the spectacle, and subsequently all the Professors and Tutors, and many students of the college, were witnesses of the phenomena. Gradually the northern streamers increased both in number and in length, as new ones sprung east and west of those originally observed. Stars could be seen dimly shining through them. The colour of these corruscations was of a blueish white near their base: farther up it was of a brighter and more silvery hue. Those nearest the moon, which was then in her first quarter, and gave a strong light, assumed for a very short time a pale green, then a bright orange colour; and one which shot up to a great length, became particularly remarkable by its brilliant redness. The whole scene was still farther enlivened by a beautiful play of crimson light gracefully undulating upwards along the streamers. The long rays continued to shoot up higher and higher, until they all converged at a point on or near the meridian, about midway between Arcturus and B Leonis. The right ascension of the focus, was found, on reference to a globe, to be 194° 20′, and its declension 18° north. It is not, however, pretended that its position was determined with perfect accuracy. At this point, the streamers which magnificently decorated the whole northern hemisphere, reddening as they converged, formed a superb oval crown of deep crimson light. This crown, which seemed like a lake of blood, extended east and west, about fifteen degrees, and ten or twelve in the opposite direction. It had such a preturnatural aspect, and, viewed in connexion with the accompanying phenomena, one of such overpowering sublimity, as to inspire a profound feeling of religious awe. It lasted from about five minutes before Eleven o'Clock, until five minutes after. Gradually the redness faded away; the corruscations, which had lately met and mingled in the colour of blood, no longer entirely converged; round the focus was left a black space of very irregular outline; south of it were seen the broken off extremities of the most northern rays: while all the rays near the convergence, had a peculiar brushy appearance. At the latter periods, the point of convergence, as well as it could be determined, was found nearer and nearer to Arcturus, indicating that the whole meter

moved with the earth. During the most brilliant stages of the phenomenon, the stars looked very dim; and the moon, previous to her setting, shorn of more than half her lustre, had a sickly, pallid aspect. For the space of two hours after the disappearing of the Auroral crown, the illuminated portion of the heavenly dome, exhibited in great brilliancy and variety, the phenomenon fancifully called the "merry dancers." It was the incessant play of a flickering light, not so bright as the Vespertine, which, in some respects it resembled, glancing about in various directions, but chiefly towards the Zenith, over the vast expanse. Its motions were far too varied and fantastic, to admit of description. In general, one flash seemed to chase another, as they rose in graceful undulations, or rather darted up the sky, along and between the white auroral columns. At half-past eleven, the spectacle began to lose its attractive brilliancy. Still later, the luminous rays were intersected by two irregular belts of white vapour, which appeared successively in the north and north-east, one of them spanning an arc of about thirty, the other of forty or forty-five degrees. There were a few other nebulous masses of the same substance, but of less extent. About twelve o'Clock, the merry dancers renewed the splendour of their exhibition, and continued it with less and less brilliancy for an hour. Between eleven and twelve, a dark cloud had arisen in the north east; before two, the wind was blowing from the south, and the sky was so far overcast, as to hide completely from our view all that remained of that magnificence and splendour, on which we had gazed for hours, and would willingly have gazed at, much longer.

"The magnetic needle was observed to oscillate during the phenomenon. Its perceived variation was forty-five seconds westward; but we have reason to suppose that a greater variation might have been detected, had we been able to ascertain it with more perfect accuracy.

"The morning of the 23d was cloudy, with a very high south-east wind. The thermometer at sunrise, stood at 42°, the barometer at 29·98. It was fair at noon. At three o'Clock P. M. the thermometer indicated 62°, the barometer 29·83. The wind had now fallen, and a perfect calm ensued. At half-past eight, P. M. of the same day, a streak of red light was seen towards the north. Later, there were a few other faint indications of an aurora; but the hopes excited by them were disappointed." *Silliman's Journal.*

THE ANNALS

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JULY, 1837.

LIX. *Experimental Researches in Electricity.—Seventh Series. By* MICHAEL FARADAY, *D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution. Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.*

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§ 11. *On Electro-chemical Decomposition continued.* ¶ iv. *On some general conditions of Electro-decomposition.* ¶ v. *On a new Measurer of Volta-electricity.* ¶ vi. *On the primitive or secondary character of bodies evolved in Electro-decomposition.* ¶ vii. *On the definite nature and extent of Electro-chemical Decompositions.* § 13. *On the absolute quantity of Electricity associated with the particles or atoms of Matter.*

(*Continued from page* 326.)

746. The nature of the substances evolved will often lead to a correct judgment of their primary or secondary character, but is not sufficient alone to establish that point. Thus, nitrogen is said to be attracted sometimes by the positive and sometimes by the negative electrode, according to the bodies with which it may be combined (554. 555.), and it is on such occasions evidently viewed as a primary result*; but I think I shall show, that, when it appears at the positive electrode, or rather at the *anode*, it is a secondary result. (748.) Thus, also, Sir HUMPHRY DAVY,† and with him the great body of chemical philosophers, (including myself,) have given the appearance of copper, lead, tin, silver, gold, &c., at the negative electrode, when their aqueous solutions were acted upon by the voltaic current, as proofs that the metals, as a class, were

* Annales de Chimie; 1804; tom. li. p. 172.
† Elements of Chemical Philosophy, pp. 144. 161.

attracted to that surface; thus assuming the metal in each case to be a primary result. These, however, I expect to prove, are all secondary results; the mere consequence of chemical action, and no proofs of the attraction or the law announced.*

747. But when we take to our assistance the law of *constant electro-chemical action* already proved with regard to water (732.), and which I hope to extend satisfactorily to all bodies (821.), and consider the *quantities* as well as the *nature* of the substances set free, a generally accurate judgment of the primary or secondary character of the results may be formed: and this important point, so essential to the theory of electro-decomposition, since it decides what are the particles directly under the influence of the current, (distinguishing them from such as are not affected,) and what are the results to be expected, may be established with such degree of certainty as to remove innumerable ambiguities and doubtful considerations from this branch of the science.

748. Let us apply these principles to the case of ammonia, and the supposed determination of nitrogen to one or the other *électrode* (554. 555.). A pure strong solution of ammonia is as bad a conductor, and therefore as little liable to electro-decomposition, as pure water; but when sulphate of ammonia is dissolved in it, the whole becomes a conductor; nitrogen *almost* and occasionally *quite* pure is evolved at the *anode*, and hydrogen at the *cathode;* the ratio of the volume of the former to that of the latter varying, but being as 1 to about 3 or 4. This result would seem at first to imply that the electric current had decomposed ammonia, and that the nitrogen had been determined towards the positive electrode. But when the electricity used was measured out by the volta-electrometer (707. 736.), it was found that the hydrogen obtained was exactly in the proportion which would have been supplied by decomposed water, whilst the nitrogen had no certain or constant relation whatever. When, upon multiplying experiments, it was found that, by using a stronger or weaker solution, or a more or less powerful battery, the gas evolved at the *anode* was a mixture of oxygen and nitrogen, varying both in proportion and absolute quantity, whilst the

* It is remarkable that up to 1804 it was the received opinion that the metals were reduced by the nascent hydrogen. At that date the general opinion was reversed by Hisinger and Berzelius (Annales de Chimie, 1804, tom. li. p. 174.), who stated that the metals were evolved directly by the electricity: in which opinion it appears, from that time, Davy coincided. (Philosophical Transactions, 1826, p. 388.).

hydrogen at the *cathode* remained constant, no doubt could be entertained that the nitrogen at the *anode* was a secondary result, depending upon the chemical action of the nascent oxygen, determined to that surface by the electric current, upon the ammonia in solution. It was the water, therefore, which was electrolyzed, not the ammonia. Further, the experiment gives no real indication of the tendency of the element nitrogen to either one electrode or the other; nor do I know of any experiment with nitric acid, or other compounds of nitrogen, which shows the tendency of this element, under the influence of the electric current, to pass in either direction along its course.

749. As another illustration of secondary results, the effects on a solution of acetate of potassa may be quoted. When a very strong solution was used, more gas was evolved at the *anode* than at the *cathode*, in the proportion of 4 to 3 nearly: that from the *anode* was a mixture of carbonic oxide and carbonic acid; that from the *cathode* pure hydrogen. When a much weaker solution was used, less gas was evolved at the *anode* than at the *cathode*; and it now contained carburetted hydrogen, as well as carbonic oxide and carbonic acid. This result of carburetted hydrogen at the positive electrode has a very anamolous appearance, if considered as an immediate consequence of the decomposing power of the current. It, however, as well as the carbonic oxide and acid, is only a *secondary result*; for it is the water alone which suffers electro-decomposition, and it is the oxygen eliminated at the *anode* which, reacting on the acetic acid, in the midst of which it is evolved, produces those substances that finally appear there. This is fully proved by experiments with the volta-electrometer (707.); for then the hydrogen evolved from the acetate at the *cathode* is always found to be definite, being exactly proportionate to the electricity which has passed through the solution, and, in quantity, the same as the hydrogen evolved in the volta-electrometer itself. The appearance of the carbon in combination with the hydrogen at the positive electrode, and its non-appearance at the negative electrode, are in curious contrast with the results which might have been expected from the law usually accepted respecting the final places of the elements.

750. If the salt in solution be an acetate of lead, then the results at both electrodes are secondary, and cannot be used to estimate or express the amount of electro-chemical action, except by a circuitous process (843.). In place of oxygen, or even the gases already described (749.), peroxide of lead now appears at the positive, and lead itself at the negative elec-

trode. When other metallic solutions are used, containing, for instance, peroxides, as that of copper, combined with this or any other decomposable acid, still more complicated results will be obtained; which, viewed as direct results of the electro-chemical action, will, in their proportions, present nothing but confusion, but will appear perfectly harmonious and simple if they be considered as secondary results, and will accord in their proportions with the oxygen and hydrogen evolved from water by the action of a definite quantity of electricity.

751. I have experimented upon many bodies, with a view to determine whether the results were primary or secondary. I have been surprised to find how many of them, in ordinary cases, are of the latter class, and how frequently water is the only body electrolyzed in instances where other substances have been supposed to give way. Some of these results I will give in as few words as possible.

752. *Nitric acid.*—When very strong, it conducted well, and yielded oxygen at the positive electrode. No gas appeared at the negative electrode; but nitrous acid, and apparently nitric oxide, were formed there, which, dissolving, rendered the acid yellow or red, and at last even effervescent, from the spontaneous separation of nitric oxide. Upon diluting the acid with its bulk or more of water, gas appeared at the negative electrode. Its quantity could be varied by variations either in the strength of the acid or of the voltaic current: for that acid from which no gas separated at the *cathode,* with a weak voltaic battery, did evolve gas there with a stronger; and that battery which evolved no gas there, with a strong acid, did cause its evolution with an acid more dilute. The gas at the *anode* was always oxygen; that at the *cathode* hydrogen. When the quantity of products was examined by the volta-electrometer (707.), the oxygen, whether from strong or weak acid, proved to be in the same proportion as from water. When the acid was diluted to specific gravity 1·24, or less, the hydrogen also proved to be the same in quantity as from water. Hence I conclude that the nitric acid does not undergo electro-chemical decomposition, but the water only; that the oxygen at the *anode* is always a primary result, but that the products at the *cathode* are often secondary, and due to the reaction of the hydrogen upon the nitric acid.

753. *Nitre.*—A solution of this salt yields very variable results, according as one or other form of tube is used, or as the electrodes are large or small. Sometimes the whole of the hydrogen of the water decomposed may be obtained at the negative electrode; at other times, only a part of it, be-

cause of the ready formation of secondary results. The solution is a very excellent conductor of electricity.

754. *Nitrate of ammonia*, in aqueous solution, gives rise to secondary results very varied and uncertain in their proportions.

755. *Sulphurous acid.*—Pure liquid sulphurous acid does not conduct nor suffer decomposition by the voltaic current*, but, when dissolved in water, the solution acquires conducting power, and is decomposed, yielding oxygen at the *anode*, and hydrogen and sulphur at the *cathode*.

756. A solution containing sulphuric acid in addition, was a better conductor. It gave very little gas at either electrode: that at the *anode* was oxygen, that at the *cathode* pure hydrogen. From the *cathode* also rose a white turbid stream, consisting of diffused sulphur, which soon rendered the whole solution milky. The volumes of gases were in no regular proportion to the quantities evolved from water in the volta-electrometer. I conclude that the sulphurous acid was not at all affected by the electric current in any of these cases, and that the water present was the only body electro-chemically decomposed; that, at the *anode*, the oxygen from the water converted the sulphurous acid into sulphuric acid, and, at the *cathode*, the hydrogen electrically evolved decomposed the sulphurous acid, combining with its oxygen, and setting its sulphur free. I conclude that the sulphur at the negative electrode was only a secondary result; and, in fact, no part of it was found combined with the small portion of hydrogen which escaped when weak solutions of sulphurous acid were used.

757. *Sulphuric acid.*—I have already given my reasons for concluding that sulphuric acid is not electrolyzable, i. e. not decomposable directly by the electric current, but occasionally suffering by a secondary action at the *cathode* from the hydrogen evolved there (681.). In the year 1800, Davy considered the sulphur from sulphuric acid as the result of the action of the nascent hydrogen†. In 1804, Hisinger and Berzelius stated that it was the direct result of the action of the voltaic pile‡; an opinion which from that time Davy seems to have adopted, and which has since been commonly received by all. The change of my own opinion requires that I should correct what I have already said of the decomposition

* See also De la Rive, Bibliothèque Universelle, tom. xl. p. 205; or Quarterly Journal of Science, vol. xxvii. p. 407.

† Nicholson's Quarterly Journal, vol. iv. pp. 280, 281.

‡ Annales de Chimie, 1804, tom. li. p. 173.

of sulphuric acid in a former series of these Researches (552.): I do not now think that the appearance of the sulphur at the negative electrode is an immediate consequence of electrolytic action.

758. *Muriatic acid.*—A strong solution gave hydrogen at the negative electrode, and chlorine only at the positive electrode; of the latter, a part acted on the platina and a part was dissolved. A minute bubble of gas remained; it was not oxygen, but probably air previously held in solution.

759. It was an important matter to determine whether the chlorine was a primary result, or only a secondary product, due to the action of the oxygen evolved from water at the *anode* upon the muriatic acid; i. e. whether the muriatic acid was electrolyzable, and if so, whether the decomposition was *definite*.

760. The muriatic acid was gradually diluted. One part with six of water gave only chlorine at the *anode*. One part with eight of water gave only chlorine; with nine of water, a little oxygen appeared with the chlorine: but the occurrence or non-occurrence of oxygen at these strengths depended, in part, on the strength of the voltaic battery used. With fifteen parts of water, a little oxygen, with much chlorine, was evolved at the *anode*. As the solution was now becoming a bad conductor of electricity, sulphuric acid was added to it; this caused more ready decomposition, but did not sensibly alter the proportion of chlorine and oxygen.

761. The muriatic acid was now diluted with 100 times its volume of dilute sulphuric acid. It still gave a large proportion of chlorine at the *anode*, mingled with oxygen; and the result was the same, whether a voltaic battery of 40 pairs of plates or one containing only 5 pairs were used. With acid of this strength, the oxygen evolved at the *anode* was to the hydrogen at the *cathode*, in volume, as 17 is to 64; and therefore the chlorine would have been 30 volumes, had it not been dissolved by the fluid.

762. Next, with respect to the quantity of elements evolved. On using the volta-electrometer, it was found that, whether the strongest or the weakest muriatic acid were used, whether chlorine alone or chlorine mingled with oxygen appeared at the *anode*, still the hydrogen evolved at the *cathode* was a constant quantity, i. e. exactly the *same* as the hydrogen which the *same quantity of electricity* could evolve from water.

763. This constancy does not decide whether the muriatic acid is electrolyzed or not, although it proves that if so, it must be in definite proportions to the quantity of electricity

used. Other considerations may, however, be allowed to decide the point. The analogy between chlorine and oxygen, in their relations to hydrogen, is so strong, as to lead almost to the certainty, that, when combined with that element, they would perform similar parts in the process of electro-decomposition. They both unite with it in single proportional or equivalent quantities; and, the number of proportionals appearing to have an intimate and important relation to the decomposability of a body (697.), those in muriatic acid, as well as in water, are the most favourable, or those, perhaps even necessary, to decomposition. In other binary compounds of chlorine also, where nothing equivocal depending on the simultaneous presence of it and oxygen is involved, the chlorine is directly eliminated at the *anode* by the electric current. Such is the case with the chloride of lead (395.), which may be justly compared with protoxide of lead (402.), and stands in the same relation to it as muriatic acid to water. The chlorides of potassium, sodium, barium, &c., are in the same relation to the protoxides of the same metals, and present the same results under the influence of the electric current (402).

764. From all the experiments, combined with these considerations, I conclude that muriatic acid is decomposed by the direct influence of the electric current, and that the quantities evolved are, and therefore the chemical action is, *definite for a definite quantity of electricity.* For though I have not collected and measured the chlorine, in its separate state, at the *anode*, there can exist no doubt as to its being proportional to the hydrogen at the *cathode*; and the results are therefore sufficient to establish the general law of *constant electro-chemical action* in the case of muriatic acid.

765. In the dilute acid (761.), I conclude that a part of the water is electro-chemically decomposed, giving origin to the oxygen, which appears mingled with the chlorine at the *anode*. The oxygen may be viewed as a secondary result; but I incline to believe that it is not so: for, if it were, it might be expected in largest proportion from the stronger acid, whereas the reverse is the fact. This consideration, with others, also leads me to conclude that muriatic acid is more easily decomposed by the electric current than water; since, even when diluted with eight or nine times its quantity of the latter fluid, it alone gives way, the water remaining unaffected.

766. *Chlorides.*—On using solutions of chlorides in water, for instance, the chlorides of sodium or calcium, there was evolution of chlorine only at the positive electrode, and of hydrogen, with the oxide of the base, as soda or lime, at the negative electrode. The process of decomposition may be

viewed as proceeding in two or three ways, all terminating in the same results. Perhaps the simplest is to consider the chloride as the substance electrolyzed, its chlorine being determined to and evolved at the *anode*, and its metal passing to the *cathode*, where, finding no more chlorine, it acts upon the water, producing hydrogen and an oxide as secondary results. As the discussion would detain me from more important matter, and is not of immediate consequence, I shall defer it for the present. It is, however, of *great consequence* to state, that, on using the volta-electrometer, the hydrogen in both cases was definite; and if the results do not prove the definite decomposition of chlorides, (which shall be proved elsewhere, 789. 794. 814.,) they are not in the slightest degree opposed to such a conclusion, and do support the *general law*.

767. *Hydriodic acid.*—A solution of hydriodic acid was affected exactly in the same manner as muriatic acid. When strong, hydrogen was evolved at the negative electrode, in definite proportion to the quantity of electricity which had passed, i. e. in the same proportion as was evolved by the same current from water; and iodine without any oxygen was evolved at the positive electrode. But when diluted, small quantities of oxygen appeared with the iodine at the *anode*, the proportion of hydrogen at the *cathode* remaining undisturbed.

768. I believe the decomposition of the hydriodic acid in this case to be direct, for the reasons already given respecting muriatic acid (763. 764.).

769. *Iodides.*—A solution of iodide of potassium being subjected to the voltaic current, iodine appeared at the positive electrode (without any oxygen), and hydrogen with free alkali at the negative electrode. The same observations as to the mode of decomposition are applicable here as were made in relation to the chlorides when in solution (766.).

770. *Hydro-fluoric acid and fluorides.*—Solution of hydro-fluoric acid did not appear to be decomposed under the influence of the electric current; it was the water which gave way apparently. The fused fluorides were electrolyzed (417.); but having during these actions obtained *fluorine* in the separate state, I think it better to refer to a future series of these Researches, in which I purpose giving a fuller account of the results than would be consistent with propriety here.

771. *Hydro-cyanic acid* in solution conducts very badly. The definite proportion of hydrogen (equal to that from water) was set free at the *cathode*, whilst at the *anode* a small quantity of oxygen was evolved and apparently a solution of cyanogen formed. The action altogether corresponded with that

on a dilute muriatic or hydriodic acid. When the hydrocyanic acid was made a better conductor by sulphuric acid, the same results occurred.

Cyanides.—With a solution of the cyanide of potassium, the result was precisely the same as with a chloride or iodide. No oxygen was evolved at the positive electrode, but a brown solution formed there. For the reasons given when speaking of the chlorides (766.), and because a fused cyanide of potassium evolves cyanogen at the positive electrode,* I incline to believe that the cyanide in solution is *directly* decomposed.

772. *Ferro-cyanic acid* and the *ferro-cyanides*, as also *sulpho-cyanic acid* and the *sulpho-cyanides*, presented results corresponding with those just described (771.).

773. *Acetic acid.* Glacial acetic acid, when fused (405.), is not decomposed by, nor does it conduct, electricity. On adding a little water to it, still there were no signs of action; on adding more water, it acted slowly and about as water alone would do. Dilute sulphuric acid was added to it in order to make it a better conductor; then the definite proportion of hydrogen was evolved at the *cathode*, and a mixture of oxygen in very deficient quantity, with carbonic acid, and a little carbonic oxide, at the *anode*. Hence it appears that acetic acid is not electrolyzable, but that a portion of it is decomposed by the oxygen evolved at the *anode*, producing secondary results, varying with the strength of the acid, the intensity of the current, and other circumstances.

774. *Acetates.*—One of these has been referred to already, as affording only secondary results relative to the acetic acid (749.). With many of the metallic acetates the results at both electrodes are secondary (746. 750.).

Acetate of soda fused and anhydrous is directly decomposed, being, as I believe, a true electrolyte, and evolving soda and acetic acid at the *cathode* and *anode*. These, however, have no sensible duration, but are immediately resolved into other substances; charcoal, sodiuretted hydrogen, &c., being set free at the former, and as far as I could judge under the circumstances, acetic acid mingled with carbonic oxide, carbonic acid, &c., at the latter.

775. *Tartaric acid.*—Pure solution of tartaric acid is almost as bad a conductor as pure water. On adding sulphuric acid to it, it conducted well, the results at the positive elec-

* It is a very remarkable thing to see carbon and nitrogen in this case determined powerfully towards the positive surface of the voltaic battery; but it is perfectly in harmony with the theory of electro-chemical decomposition which I have advanced.

trode being primary or secondary in different proportions, according to variations in the strength of the acid and the power of the electric current (752.). Alkaline tartrates gave a large proportion of secondary results at the positive electrode. The hydrogen at the negative electrode remained constant unless certain metallic salts were used.

776. Solutions of salts containing other vegetable acids, as the benzoates; of sugar, gum, &c., dissolved in dilute sulphuric acid; of resin, albumen, &c., dissolved in alkalies, were in turn submitted to the electrolytic power of the voltaic current. In all these cases, secondary results to a greater or smaller extent were produced at the positive electrode.

777. In concluding this division of these Researches, it cannot but occur to the mind that the final result of the action of the electric current upon substances placed between the electrodes, instead of being simple may be very complicated. There are two modes by which these substances may be decomposed, either by the direct force of the electric current, or by the action of bodies which that current may evolve. There are also two modes by which new compounds may be formed, i. e. by combination of the evolving substances whilst in their nascent state (658.), directly with the matter of the electrode; or else their combination with those bodies, which being contained in, or associated with, the decomposing conductor, are necessarily present at the *anode* and *cathode*. The complexity is rendered still greater by the circumstance that two or more of these actions may occur simultaneously, and also in variable proportions to each other. But it may in a great measure be resolved by attention to the principles already laid down (747.).

778. When *aqueous* solutions of bodies are used, secondary results are exceedingly frequent. Even when the water is not present in large quantity, but is merely that of combination, still secondary results often ensue; for instance, it is very possible that in Sir Humphry Davy's decomposition of the hydrates of potassa and soda, a part of the potassium produced was the result of a secondary action. Hence, also, a frequent cause for the disappearance of the oxygen and hydrogen which would otherwise be evolved: and when hydrogen does *not* appear at the *cathode* in an *aqueous solution*, it perhaps always indicates that a secondary action has taken place there. No exception to this rule has as yet occurred to my observation.

779. Secondary actions are *not confined to aqueous solutions*, or cases where water is present. For instance, various chlorides acted upon when fused (402.), by platina electrodes,

have the chlorine determined electrically to the *anode*. In many cases, as with chlorides of lead, potassium, barium, &c., the chlorine acts on the platina and forms a compound with it, which dissolves; but when protochloride of tin is used, the chlorine at the *anode* does not act upon the platina, but upon the chloride already there, forming a perchloride which rises in vapour (790. 804.). These are, therefore, instances of secondary actions of both kinds, produced in bodies containing no water.

780. The production of boron from fused borax (402. 417.) is also a case of secondary action; for boracic acid is not decomposable by electricity (408.), and it was the sodium evolved at the *cathode* which, reacting on the boracic acid around it, took oxygen from it and set boron free in the experiments formerly described.

781. Secondary actions have already, in the hands of M. Becquerel, produced many interesting results in the formation of compounds: some of them new, others imitations of those occurring naturally*. It is probable they may prove equally interesting in an opposite direction, i. e. as affording cases of analytic decomposition. Much information regarding the composition, and perhaps even the arrangement of the particles of such bodies as the vegetable acids and alkalies, and organic compounds generally, will probably be obtained by submitting them to the action of nascent oxygen, hydrogen, chlorine, &c., at the electrodes; and the action seems the more promising, because of the thorough command which we possess over attendant circumstances, such as the strength of the current, the size of the electrodes, the nature of the decomposing conductor, its strength, &c., all of which may be expected to have their corresponding influence upon the final result.

782. It is to me a great satisfaction that the extreme variety of secondary results have presented nothing opposed to the doctrine of a constant and definite electro-chemical action, to the particular consideration of which I shall now proceed.

¶ vii. *On the definite nature and extent of Electro-chemical Decomposition.*

783. In the third series of these Researches, after proving the identity of electricities derived from different sources, and showing, by actual measurement, the extraordinary quantity

* Annales de Chimie, tom. xxxv. p. 113.

of electricity evolved by a very feeble voltaic arrangement (371. 376.), I announced a law, derived from experiment, which seemed to me of the utmost importance to the science of electricity in general, and that branch of it denominated electro-chemistry in particular. The law was expressed thus: *The chemical power of a current of electricity is in direct proportion to the absolute quantity of electricity which passes* (377.).

784. In the further progress of the successive investigations, I have had frequent occasion to refer to the same law, occasionally in circumstances offering powerful corroboration of its truth (456. 504. 505.); and the present series already supplies numerous new cases in which it holds good (704. 722. 726. 732.). It is now my object to consider this great principle more closely, and to develope some of the consequences to which it leads. That the evidence for it may be the more distinct and applicable, I shall quote cases of decomposition subject to as few interferences from secondary results as possible, effected upon bodies very simple, yet very definite in their nature.

785. In the first place, I consider the law as so fully established with respect to the decomposition of *water*, and under so many circumstances which might be supposed, if anything could, to exert an influence over it, that I may be excused entering into further detail respecting that substance, or even summing up the results here (732). I refer, therefore, to the whole of the subdivision of this series of Researches which contains the account of the *volta-electrometer*.

786. In the next place, I also consider the law as established with respect to *muriatic acid* by the experiments and reasoning already advanced, when speaking of that substance, in the subdivision respecting primary and secondary results (758, &c.).

787. I consider the law as established also with regard to *hydriodic acid* by the experiments and considerations already advanced in the preceding division of this series of Researches (767. 768.).

788. Without speaking with the same confidence, yet from the experience described, and many others not described, relating to hydro-fluoric, hydro-cyanic, ferro-cyanic, and sulpho-cyanic acids (770. 771. 772.), and from the close analogy which holds between these bodies and the hydro-acids of chlorine, iodine, bromine, &c., I consider these also as coming under subjection to the law, and assisting to prove its truth.

789. In the preceding cases, except the first, the water is

believed to be inactive; but to avoid any ambiguity arising from its presence, I sought for substances from which it should be absent altogether; and, taking advantage of the law of conduction already developed (380. &c.), soon found abundance, amongst which *protochloride of tin* was first subjected to decomposition in the following manner. A piece of platina wire had one extremity coiled up into a small knob, and having been carefully weighed, was sealed hermetically into a piece of bottle-glass tube, that the knob should be at the bottom of the tube within (fig. 82. pl. xi.). The tube was suspended by a piece of platina wire, so that the heat of a spirit-lamp could be applied to it. Recently fused protochloride of tin was introduced in sufficient quantity to occupy, when melted, about one half of the tube; the wire of the tube was connected with a volta-electrometer (711.), which was itself connected with the negative end of a voltaic battery; and a platina wire connected with the positive end of the same battery was dipped into the fused chloride in the tube; being, however, so bent, that it could not by any shake of the hand or apparatus touch the negative electrode at the bottom of the vessel. The whole arrangement is delineated fig. 91.

790. Under these circumstances the chloride of tin was decomposed: the chlorine evolved at the positive electrode formed bichloride of tin (779.), which passed away in fumes, and the tin evolved at the negative electrode combined with the platina, forming an alloy, fusible at the temperature to which the tube was subjected, and therefore never occasioning metallic communication entirely through the decomposing chloride. When the experiment had been continued so long as to yield a reasonable quantity of gas in the volta-electrometer, the battery connexion was broken, the positive electrode removed, and the tube and remaining chloride allowed to cool. When cold, the tube was broken open, the rest of the chloride and the glass being easily separable from the platina wire and its button of alloy. The latter when washed was then reweighed, and the increase gave the weight of the tin reduced.

791. I will give the particular results of one experiment, in illustration of the mode adopted in this and others, the results of which I shall have occasion to quote. The negative electrode weighed at first 20 grains; after the experiment it, with its button of alloy, weighed 23·2 grains. The tin evolved by the electric current at the *cathode* weighed, therefore, 3·2 grains. The quantity of oxygen and hydrogen collected in the volta-electrometer = 3·85 cubic inches. As 100 cubic inches of oxygen and hydrogen, in the proportions to form

water, may be considered as weighing 12·92 grains, the 3·85 cubic inches would weigh 0·49742 of a grain; that being, therefore, the weight of water decomposed by the same electric current as was able to decompose such weight of protochloride of tin as could yield 3·2 grains of metal. Now 0·49742 : 3·2 : : 9 the equivalent of water is to 57·9, which should therefore be equivalent of tin, if the experiment had been made without the error, and if the electro-chemical decomposition *is in this case also definite.* In some chemical works 58 is given as the chemical equivalent of tin, in others 57·9. Both are so near to the result of the experiment, and the experiment itself is so subject to slight causes of variation (as from the absorption of gas in the volta-electrometer (716.), &c.), that the numbers leave little doubt of the applicability of the *law of definite action* in this and all similar cases of electro-decomposition.

792. It is not often I have obtained an accordance in numbers so near as that I have just quoted. Four experiments were made on the protochloride of tin, the quantities of gas evolved in the volta-electrometer being from 2·05 to 10·29 cubic inches. The average of the four experiments gave 58·53 as the electro-chemical equivalent for tin.

793. The chloride remaining after the experiment, was pure protochloride of tin; and no one can doubt for a moment that the equivalent of chlorine had been evolved at the *anode*, and having formed bichloride of tin as a secondary result, had passed away.

794. *Chloride of lead* was experimented upon in a manner exactly similar, except that a change was made in the nature of the positive electrode; for as the chlorine evolved at the *anode* forms no perchloride of lead, but acts directly upon the platina, if that metal be used, it produces a solution of chloride of platina in the chloride of lead; in consequence of which a portion of platina can pass to the *cathode*, and will produce a vitiated result. I therefore sought for, and found in plumbago, another substance, which could be used safely as the positive electrode in such bodies as chlorides, iodides, &c. The chlorine or iodine does not act upon it, but is evolved in the free state; and the plumbago has no reaction, under the circumstances, upon the fused chloride or iodide in which it is plunged. Even if a few particles of plumbago should separate by the heat or the mechanical action of the envolved gas, they can do no harm in the chloride.

795. The mean of three experiments gave the number of 100·85 as the equivalent for lead. The chemical equivalent is 103·5. The deficiency in my experiments I attribute to

the solution of part of the gas (716.) in the volta-electrometer; but the results leave no doubt on my mind that both the lead and the chlorine are, in this case, evolved in *definite quantities* by the action of a given quantity of electricity (814. &c.).

796. *Chloride of antimony.*—It was in endeavouring to obtain the electro-chemical equivalent of antimony from the chloride that I found reasons for the statement I have made respecting the presence of water in it in an earlier part of these Researches (690. 693. &c.).

797. I endeavoured to experiment upon the *oxide of lead* obtained by fusion and ignition of the nitrate in a platina crucible, but found great difficulty, from the high temperature required for perfect fusion, and the powerful fluxing qualities of the substance. Green glass tubes repeatedly failed. I at last fused the oxide in a small porcelain crucible, heated fully in a charcoal fire; and as it was essential that the evolution of the lead at the *cathode* should take place beneath the surface, the negative electrode was guarded by a green glass tube, fused around it in such a manner as to expose only the knob of platina at the lower end (fig. 92.), so that it could be plunged beneath the surface, and thus exclude contact of air or oxygen with the lead reduced there. A platina wire was employed for the positive electrode, that metal not being subject to any action from the oxygen evolved against it. The arrangement is given fig. 93.

798. In an experiment of this kind the equivalent for the lead came out 93·17, which was very much too small. This, I believe, was because of the small interval between the positive and negative electrodes in the oxide of lead, so that it was not unlikely that some of the froth and bubbles formed by the oxygen at the *anode* should occasionally even touch the lead reduced at the *cathode*, and re-oxidize it. When I endeavoured to correct this by having more litharge, the greater heat required to keep it all fluid caused a quicker action on the crucible, which was soon eaten through, and the experiment stopped.

799. In one experiment of this kind I used borate of lead (408. 673.). It evolves lead, under the influence of the electric current, at the *anode*, and oxygen at the *cathode*; and as the boracic acid is not either directly (408.) or incidentally decomposed during the operation, I expected a result dependent on the oxide of lead. The borate is not so violent a flux as the oxide, but it requires a higher temperature to make it quite liquid; and if not very hot, the bubbles of oxygen cling to the positive electrode, and retard the transfer of electricity. The number for lead came out 101·29, which

is so near to 103·5 as to show that the action of the current had been definite.

800. *Oxide of bismuth.*—I found this substance required too high a temperature, and acted too powerfully as a flux, to allow of any experiment being made on it, without the application of more time and care than I could give at present.

801. The ordinary *protoxide of antimony*, which consists of one proportional of metal and one and a half of oxygen, was subjected to the action of the electric current in a green glass tube (789.), surrounded by a jacket of platina foil, and heated in a charcoal fire. The decomposition began and proceeded very well at first, apparently indicating, according to the general law (679. 697.), that this substance was one containing such elements and in such proportions as made it amenable to the power of the electric current. This effect I have already given reasons for supposing may be due to the presence of a true protoxide, consisting of single proportionals (696. 693.). The action soon diminished, and finally ceased, because of the formation of a higher oxide of the metal at the positive electrode. This compound, which was probably the peroxide, being infusible and insoluble in the protoxide, formed a crystalline crust around the positive electrode; and thus insulating it, prevented the transmission of the electricity. Whether if it had been fusible and still immiscible it would have decomposed, is doubtful, because of its departure from the required composition (697.). It was a very natural secondary product at the positive electrode (779.). On opening the tube it was found that a little antimony had been separated at the negative electrode; but the quantity was too small to allow of any quantitative result being obtained.

802. *Iodide of lead.*—This substance can be experimented with in tubes heated by a spirit-lamp (789.); but I obtained no good results from it, whether I used positive electrodes of platina or plumbago. In two experiments the numbers for the lead came out only 75·46 and 73·45, instead of 103·5. This I attribute to the formation of a periodide at the positive electrode, which dissolving in the mass of liquid iodide, came in contact with the lead evolved at the negative electrode, and dissolved part of it, becoming itself again protiodide. Such a periodide does exist; and it is very rarely that the iodide of lead formed by precipitation, and well washed, can be fused without evolving much iodine, from the presence of this percompound; nor does crystallization from its hot aqueous solution free it from this substance. Even when a little of the protiodide and iodine are merely rubbed

together in a mortar, a portion of the periodide is formed. And though it is decomposed by being fused and heated to dull redness for a few minutes, and the whole reduced to protiodide, yet that is not at all opposed to the possibility, that a little of that which is formed in great excess of iodine at the *anode*, should be carried by the rapid currents in the liquid into contact with the *cathode*.

803. This view of the results was strengthened by a third experiment, where the space between the electrodes was increased to one third of an inch; for now the interfering effects were much diminished, and the number of the lead came out 89.04; and it was fully confirmed by the results obtained in the cases of transfer to be immediately described (818.).

The experiments on iodide of lead, therefore, offer no exception to the *general law* under consideration, but, on the contrary, may, from general considerations, be admitted as included in it.

804. *Protiodide of tin.*—This substance, when fused (402.), conducts and is decomposed by the electric current, tin is evolved at the *anode*, and periodide of tin as a secondary result (779. 790.) at the *cathode*. The temperature required for its fusion is too high to allow of the production of any results fit for weighing.

805. *Iodide of potassium* was subjected to electrolytic action in a tube, fig. 82. (789.). The negative electrode was a globule of lead, and I hoped in this way to retain the potassium, and obtain results that could be weighed and compared with the volta-electrometer indication; but the difficulties dependent upon the high temperature required, the action upon the glass, the fusibility of the platina induced by the presence of the lead, and other circumstances, prevented me from obtaining such results. The iodide was decomposed with the evolution of iodine at the *anode*, and of potassium at the *cathode*, as in former cases.

806. In some of these experiments several substances were placed in succession, and decomposed simultaneously by the same electric current: thus, protochloride of tin, chloride of lead, and water, were thus acted on at once. It is needless to say that the results were comparable, the tin, lead, chlorine, oxygen, and hydrogen evolved being definite in quantity and electro-chemical equivalents to each other.

807. Let us turn to another kind of proof of the *definite chemical action of electricity*. If any circumstances could be supposed to exert an influence over the quantity of the matters evolved during electrolytic action, one would expect

them to be present when electrodes of different substances, and possessing very different chemical affinities for the evolving bodies, were used. Platina has no power in dilute sulphuric acid of combining with the oxygen at the *anode*, though the latter be evolved in the nascent state against it. Copper, on the other hand, immediately unites to the oxygen, as the electric current sets it free from the hydrogen; and zinc is not only able to combine with it, but can, without any help from the electricity, abstract it directly from the water, at the same time setting torrents of hydrogen free. Yet in cases where these three substances were used as the positive electrodes in three similar portions of the same dilute sulphuric acid, specific gravity 1·336, precisely the same quantity of water was decomposed by the electric current, and precisely the same quantity of hydrogen set free at the *cathodes* of the three solutions.

808. The experiment was made thus. Portions of the dilute sulphuric acid were put into three basins. Three volta-electrometer tubes of the form figg. 83,85. were filled with the same acid, and one inverted in each basin (707.). A zinc plate connected with the positive end of a voltaic battery, was dipped into the first basin, forming the positive electrode there, the hydrogen, which was abundantly evolved from it by the direct action of the acid, being allowed to escape. A copper plate, which dipped into the acid of the second basin, was connected with the negative electrode of the *first* basin; and a platina plate, which dipped into the acid of the third basin, was connected with the negative electrode of the *second* basin. The negative electrode of the third basin was connected with a volta-electrometer (711.), and that with the negative end of the voltaic battery.

809. Immediately that the circuit was complete, the *electro-chemical action* commenced in all the vessels. The hydrogen still rose in, apparently, undiminished quantities from the positive zinc electrode in the first basin. No oxygen was evolved at the positive copper electrode in the second basin, but a sulphate of copper was formed there; whilst in the third basin the positive platina electrode evolved pure oxygen gas, and was itself unaffected. But in *all* the basins the hydrogen liberated at the *negative* platina electrodes was *the same in quantity*, and the same with the volume of hydrogen evolved in the volta-electrometer, showing that in all the vessels the current had decomposed an equal quantity of water. In this trying case therefore, the *chemical action of electricity* proved to be *perfectly definite.*

810. A similar experiment was made with muriatic acid

diluted with its bulk of water. The three positive electrodes were zinc, silver, and platina; the first being able to separate and combine with the chlorine *without* the aid of the current; the second combining with the chlorine only after the current had set it free: and the third rejecting almost the whole of it. The three negative electrodes were, as before, platina plates fixed within glass tubes. In this experiment, as in the former, the quantity of hydrogen evolved at the *cathodes* was the same for all, and the same as the hydrogen evolved in the volta-electrometer. I have already given my reasons for believing that in these experiments it is the muriatic acid which is directly decomposed by the electricity (764.); and the results prove that the quantities so decomposed are *perfectly definite* and proportionate to the quantity of electricity which has passed.

811. In this experiment the chloride of silver formed in the second basin retarded the passage of the current of electricity, by virtue of the law of conduction before described (394.), so that it had to be cleaned off four or five times during the course of the experiment; but this caused no difference between the results of that vessel and the others.

812. Charcoal was used as the positive electrode in both sulphuric and muriatic acids (808.810.); but this change produced no variation of the results. A zinc positive electrode, in sulphate of soda or solution of common salt, gave the same constancy of operation.

813. Experiments of a similar kind were then made with bodies altogether in a different state, i. e. with *fused* chlorides, iodides, &c. I have already described an experiment with fused chloride of silver, in which the electrodes were of metallic silver, the one rendered negative becoming increased and lengthened by the addition of metal, whilst the other was dissolved and eaten away by its abstraction. This experiment was repeated, two weighed pieces of silver wire being used as the electrodes, and a volta-electrometer included in the circuit. Great care was taken to withdraw the negative electrode so regularly and steadily that the crystals of reduced silver should not form a *metallic* communication beneath the surface of the fused chloride. On concluding the experiment the positive electrode was re-weighed, and its loss ascertained. The mixture of chloride of silver, and metal, withdrawn in successive portions at the negative electrode, was digested in solution of ammonia, to remove the chloride, and the metallic silver remaining also weighed: it was the reduction at the *cathode*, and exactly equalled the solution at the *anode*; and

each portion was as nearly as possible the equivalent to the water decomposed in the volta-electrometer.

814. The infusible condition of the silver at the temperature used, and the length and ramifying character of its crystals, render the above experiment difficult to perform, and uncertain in its results. I therefore wrought with a chloride of lead, using a green glass tube, formed as in fig. 94. A weighed platina wire was fused into the bottom of a small tube as before described (789.). The tube was then bent to an angle, at about half an inch distance from the closed end; and the part between the angle and the extremity being softened, was forced upward, as in the figure, so as to form a bridge, or rather separation, producing two little depressions or basins *a, b,* within the tube. This arrangement was suspended by a platina wire, as before, so that the heat of a spirit-lamp could be applied to it, such inclination being given to it as would allow all air to escape during the fusion of the chloride of lead. A positive electrode was then provided, by binding up the end of a platina wire into a knob, and fusing about twenty grains of metallic lead on to it, in a small closed tube of glass, which was afterwards broken away. Being so furnished, the wire with its knob was weighed, and the weight recorded.

815. Chloride of lead was now introduced into the tube, and carefully fused. The leaded electrode was also introduced: after which the metal, at its extremity, soon melted. In this state of things the tube was filled up to *c* with melted chloride of lead; the end of the electrode to be rendered negative was in the basin *b*, and the electrode of melted lead was retained in the basin *a*, and, by connexion with the proper conducting wire of a voltaic battery, was rendered positive. A volta-electrometer was included in the circuit.

816. Immediately upon the completion of the communication with the voltaic battery, the current passed, and decomposition proceeded. No chlorine was evolved at the positive electrode; but as the fused chloride was transparent, a button of alloy could be observed gradually forming and increasing in size at *b*, whilst the lead at *a* could also be seen gradually to diminish. After a time, the experiment was stopped; the tube allowed to cool, and broken open; the wires, with their buttons, cleaned and weighed; and their change in weight compared with the indication of the *volta-electrometer*.

817. In this experiment the positive electrode had lost just as much lead as the negative one had gained (795.), and the loss or gain was very nearly the equivalent of the water de-

composed in the volta-electrometer, giving for lead the number 101·5. It is therefore evident, in this instance, that causing a *strong affinity*, or *no affinity*, for the substance evolved at the *anode*, to be active during the experiment (807.), produces no variation in the definite action of the electric current.

818. A similar experiment was then made with iodide of lead, and in this manner all confusion from the formation of a periodide avoided (803.). No iodine was evolved during the whole action, and finally the loss of lead at the *anode* was the same as the gain at the *cathode*, the equivalent number, by comparison with the result in the volta-electrometer, being 103·5.

819. Then protochloride of tin was subjected to the electric current in the same manner, using, of course, a tin positive electrode. No bichloride of tin was now formed (779.790.). On examining the two electrodes, the positive had lost precisely as much as the negative had gained; and by comparison with the volta-electrometer, the number for tin came out 59.

820. It is quite necessary in these and similar experiments to examine the interior of the bulbs of alloy at the ends of the conducting wires; for occasionally, and especially with those which have been positive, they are cavernous, and contain portions of the chloride or iodide used, which must be removed before the final weight is ascertained. This is more usually the case with lead than tin.

821. All these facts combined into, I think, an irresistible mass of evidence, proving the truth of the important proposition which I at first laid down, namely, *that the chemical power of a current of electricity is in direct proportion to the absolute quantity of electricity which passes* (377. 783.). They prove, too, that this is not merely true with one substance, as water, but generally with all electrolytic bodies; and, further, that the results obtained with any *one substance* do not merely agree amongst themselves, but also with those obtained from *other substances*, the whole combining together into *one series of definite electro-chemical actions* (505.). I do not mean to say that no exceptions will appear: perhaps some may arise, especially amongst substances existing only by weak affinity; but I do not expect that any will seriously disturb the result announced. If, in the well considered, well examined, and, I may surely say, well ascertained doctrines of the definite nature of ordinary chemical affinity, such exceptions occur, as they do in abundance, yet, without being allowed to disturb our minds as to the general conclusion, they ought also to be allowed if they should present them-

selves at this, the opening of a new view of electro-chemical action; not being held up as obstructions to those who may be engaged in rendering that view more and more perfect, but laid aside for a while, in hopes that their perfect and consistent explanation will finally appear.

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822. The doctrine of *definite electro-chemical action* just laid down, and, I believe, established, leads to some new views of the relations and classifications of bodies associated with or subject to this action. Some of these I shall proceed to consider.

823. In the first place, compound bodies may be separated into two great classes, namely, those which are decomposable by the electric current, and those which are not. Of the latter, some are conductors, others non-conductors, of voltaic electricity*. The former do not depend for their decomposability, upon the nature of their elements only; for, of the same two elements, bodies may be formed, of which one shall belong to one class and another to the other class; but probably on the proportions also (697.). It is further remarkable, that with very few, if any, exceptions (414. 691.), these decomposable bodies are exactly those governed by the remarkable law of conduction I have before described (394.); for that law does not extend to the many compound fusible substances that are excluded from this class. I propose to call bodies of this, the decomposable class, *Electrolytes* (664.).

824. Then, again, the substances into which these divide, under the influence of the electric current, form an exceedingly important general class. They are combining bodies; are directly associated with the fundamental parts of the doctrine of chemical affinity; and have each a definite proportion, in which they are always evolved during electrolytic action. I have proposed to call these bodies generally *ions*, or particularly *anions* and *cations*, according as they appear at the *anode* or *cathode* (665.); and the numbers representing the proportions in which they are evolved *electro-chemical equivalents*. Thus hydrogen, oxygen, chlorine, iodine, lead, tin, are *ions*; the three former are *anions*, the two metals are *cations*, and 1, 8, 36, 125, 104, 58, are their *electro-chemical equivalents* nearly.

825. A summary of certain points already ascertained respecting *electrolytes*, *ions*, and *electro-chemical equivalents*,

* I mean here by voltaic electricity, merely electricity from a most abundant source, but having very small intensity.

may be given in the following general form of propositions, without, I hope, including any serious error.

826. i. A single *ion*, i. e. one not in combination with another, will have no tendency to pass to either of the electrodes, and will be perfectly indifferent to the passing current, unless it be itself a compound of more elementary *ions*, and so subject to actual decomposition. Upon this fact is founded much of the proof adduced in favour of the new theory of electro-chemical decomposition, which I put forth in a former series of these Researches (518. &c.).

827. ii. If one *ion* be combined in right proportions (697.) with another strongly opposed to it in its ordinary chemical relations, i. e. if an *anion* be combined with a *cation*, then both will travel, the one to the *anode*, the other to the *cathode*, of the decomposing body (530. 542. 547.).

828. iii. If, therefore, an *ion* pass towards one of the electrodes, another *ion* must also be passing simultaneously to the other electrode, although, from secondary action, it may not make its appearance (743.).

829. iv. A body decomposable directly by the electric current, i. e. an *electrolyte*, must consist of two *ions*, and must also render them up during the act of decomposition.

830. v. There is but one *electrolyte* composed of the same two elementary *ions*; at least such appears to be the fact (697), dependent upon a law, that *only single electro-chemical equivalents of elementary ions can go to the electrodes, and not multiples.*

831. vi. A body not decomposable when alone, as boracic acid, is not directly decomposable by the electric current when in combination (780.). It may act as an *ion*, going only to the *anode* or *cathode*, but does not yield up its elements, except occasionally by a secondary action. Perhaps it is superfluous for me to point out that this proposition has *no relation* to such cases as that of water, which, by the presence of other bodies, is rendered a better conductor of electricity, and *therefore* is more freely decomposed.

832. vii. The nature of the substance of which the electrode is formed, provided it be a conductor, causes no difference in the electro-decomposition, either in kind or degree (807. 813.); but it seriously influences, by secondary action (744.), the state in which the *ions* finally appear. Advantage may be taken of this principle in combining and collecting such *ions* as, if evolved in their free state, would be unmanageable*.

* It will often happen that the electrodes used may be of such a nature as, with the fluid in which they are immersed, to produce an

833. viii. A substance which, being used as the electrode, can combine altogether with the *ion* evolved against it, is also, I believe, an *ion*, and combines, in such cases, in the quantity represented by its *electro-chemical equivalent*. All the experiments I have made agree with this view; and it seems to me, at present, to result as a necessary consequence. Whether, in the secondary actions that take place, where the *ion* acts, not upon the matter of the electrode, but on that which is around it in the liquid (744.), the same consequence follows, will require more extended investigation to determine.

834. ix. Compound *ions* are not necessarily composed of electro-chemical equivalents of simple *ions*. For instance, sulphuric acid, boracic acid, phosphoric acid, are *ions*, but not *electrolytes*, i. e. not composed of electro-chemical equivalents of simple *ions*.

835. x. Electro-chemical equivalents are always consistent; i. e. the same number which represents the equivalent of a substance A when it is separating from a substance B, will also represent A when separating from a third substance C. Thus, 8 is the electro-chemical equivalent of oxygen, whether separating from hydrogen, or tin, or lead; and 103·5 is the electro-chemical equivalent of lead, whether separating from oxygen, or chlorine, or iodine.

836. xi. Electro-chemical equivalents coincide, and are the same, with ordinary chemical equivalents.

837. By means of experiment and the preceding propositions, a knowledge of *ions* and their electro-chemical equivalents may be obtained in various ways.

838. In the first place, they may be determined directly, as has been done with hydrogen, oxygen, lead, and tin, in the numerous experiments already quoted.

839. In the next place, from propositions ii. and iii., may be deduced the knowledge of many other *ions*, and also their equivalents. When chloride of lead was decomposed, platina being used for both electrodes (395.), there could remain no more doubt that chlorine was passing to the *anode*, although it combined with the platina there, than when the positive electrode, being of plumbago (794.), allowed its evolution in

electric current, either according with or opposing that of the voltaic arrangement used, and in this way, or by direct chemical action, may sadly disturb the results. Still, in the midst of all these confusing effects, the electric current, which actually passes in any direction through the decomposing body, will produce its own definite electrolytic action.

the free state; neither could there, in either case, remain any doubt, that for every 103.5 parts of lead evolved at the *cathode*, 36 parts of chlorine were evolved at the *anode*, for the remaining chloride of lead was unchanged. So also when in a metallic solution one volume of oxygen, or a secondary compound containing that proportion, appeared at the *anode*, no doubt could arise that hydrogen, equivalent to two volumes, had been determined to the *cathode*, although, by a secondary action, it had been employed in reducing oxides of lead, copper, or other metals, to the metallic state. In this manner then, we learn from the experiments already described in these Researches, that chlorine, iodine, bromine, fluorine, calcium, potassium, strontium, magnesium, manganese, &c., are *ions*, and that their *electro-chemical equivalents* are the same as their *ordinary chemical equivalents*.

840. Propositions iv. and v. extend our means of gaining information. For if a body of known chemical composition is found to be decomposable, and the nature of the substance evolved as a primary or even a secondary result (743. 777.) at one of the electrodes, be ascertained, the electro-chemical equivalent of that body may be deduced from the known constant composition of the substance evolved. Thus, when fused protiodide of tin is decomposed by the voltaic current (804.), the conclusion may be drawn, that both the iodine and tin are *ions*, and that the proportions in which they combine in the fused compound express their electro-chemical equivalents. Again, with respect to the fused iodide of potassium (805.), it is an electrolyte; and the chemical equivalents will also be the electro-chemical equivalents.

841. If proposition viii. sustain extensive experimental investigation, then it will not only help to confirm the results obtained by the use of the other propositions, but will give abundant original information of its own.

842. In many instances, the *secondary results* obtained by the action of the evolving *ion* on the substances present in the surrounding liquid or solution, will give the electro-chemical equivalent. Thus, in the solution of acetate of lead, and, as far as I have gone, in other proto-salts subjected to the reducing action of the nascent hydrogen at the *cathode*, the metal precipitated has been in the same quantity as if it had been a primary product, (provided no free hydrogen escaped there), and therefore gave as accurately the number representing its electro-chemical equivalent.

843. Upon this principle it is that secondary results may occasionally be used as measurers of the volta-electric current (706. 740.); but there are not many metallic solutions

that answer this purpose well: for unless the metal is easily precipitated, hydrogen will be evolved at the *cathode* and vitiate the result. If a soluble peroxide is formed at the *anode* or if the precipitated metal crystallize across the solution and touch the positive electrode, similar vitiated results are obtained. I expect to find in some vegetable salts, as the acetates of mercury and zinc, solutions favourable for this use.

844. After the first experimental investigation to establish the definite chemical action of electricity, I have not hesitated to apply the more strict results of chemical analysis to correct the numbers obtained as electrolytical results. This, it is evident, may be done in a great number of cases, without using too much liberty towards the due severity of scientific research. The series of numbers representing electro-chemical equivalents must, like those expressing the ordinary equivalents of chemically acting bodies, remain subject to the continual correction of experiment and sound reasoning.

845. I give the following brief Table of *ions* and their electro-chemical equivalents, rather as a specimen of a first attempt than as anything that can supply the want which must very quickly be felt, of a full and complete tabular account of this class of bodies. Looking forward to such a table as of extreme utility (if well constructed) in developing the intimate relation of ordinary chemical affinity to electrical actions, and identifying the two, not to the imagination merely, but to the conviction of the senses and a sound judgment, I may be allowed to express a hope, that the endeavour will always be to make it a table of *real*, and not *hypothetical*, electro-chemical equivalents; for we shall else overrun the facts, and lose all sight and consciousness of the knowledge lying directly in our path.

846. The equivalent numbers do not profess to be exact, and are taken almost entirely from the chemical results of other philosophers in whom I could repose more confidence, as to these points, than in myself.

847. Table of Ions.

Anions.

Oxygen	8	Sulphuric acid	40
Chlorine	35·5	Selenic acid	64
Iodine	126	Nitric acid	54
Bromine	78·3	Chloric acid	75·5
Fluorine	18·7	Phosphoric acid	35·7
Cyanogen	26	Carbonic acid	22

Anions.

Boracic acid	24	Oxalic acid	36
Acetic acid	51	Sulphur (?)	16
Tartaric acid	66	Selenium (?)	
Citric acid	58	Sulpho-cyanogen	

Cations.

Hydrogen	1	Mercury	200
Potassium	39·2	Silver	108
Sodium	23·3	Platina	98·6?
Lithium	10	Gold	(?)
Barium	68·7		
Strontium	43·8	Ammonia	17
Calcium	20·5	Potassa	47·2
Magnesium	12·7	Soda	31·3
Manganese	27·7	Lithia	18
Zinc	32·5	Baryta	76·7
Tin	57·9	Strontia	51·8
Lead	103·5	Lime	28·5
Iron	28	Magnesia	20·7
Copper	31·6	Alumina	(?)
Cadmium	55·8	Protoxides generally.	
Cerium	46	Quinia	171·6
Cobalt	29·5	Cinchona	160
Nickel	29·5	Morphia	290
Antimony	64·6?	Vegeto-alkalies generally	
Bismuth	71		

848. This Table might be further arranged into groups of such substance as either act with, or replace, each other. Thus, for instance, acids and bases act in relation to each other; but they do not act in association with oxygen, hydrogen, or elementary substances. There is indeed little or no doubt that, when the electrical relations of the particles of matter come to be closely examined, this division must be made. The simple substances, with cyanogen, sulpho-cyanogen, and one or two other compound bodies, will probably form the first group; and the acids and bases, with such analogous compounds as may prove to be *ions*, the second group. Whether these will include all *ions*, or whether a third class of more complicated results will be required, must be decided by future experiments.

849. It is *probable* that all our present elementary bodies are *ions*, but that is not as yet certain. There are some, such as carbon, phosphorus, nitrogen, silicon, boron, alumium, the right of which to the title of *ion* it is desirable to decide as soon as possible. There are also many compound bodies,

and amongst them alumina and silica, which it is desirable to class immediately by unexceptionable experiments. It is also *possible*, that all combinable bodies, compound as well as simple, may enter into the class of *ions;* but at present it does not seem to me probable. Still the experimental evidence I have is so small in proportion to what must gradually accumulate around, and bear upon, this point, that I am afraid to give a strong opinion upon it.

850. I think I cannot deceive myself in considering the doctrine of definite electro-chemical action as of the utmost importance. It touches by its facts more directly and closely than any former fact, or set of facts, have done, upon the beautiful idea, that ordinary chemical affinity is a mere consequence of the electrical attractions of the particles of different kinds of matter; and it will probably lead us to the means by which we may enlighten that which is at present so obscure, and either fully demonstrate the truth of the idea, or develope that which ought to replace it.

851. A very valuable use of electro-chemical equivalents will be to decide, in cases of doubt, what is the true chemical equivalent, or definite proportional, or atomic number of a body; for I have such conviction that the power which governs electro-decomposition and ordinary chemical attractions is the same; and such confidence in the overruling influence of those natural laws which render the former definite, as to feel no hesitation in believing that the latter must submit to them also. Such being the case, I can have no doubt that, assuming hydrogen as 1, and dismissing small fractions for the simplicity of expression, the equivalent number or atomic weight of oxygen is 8, of chlorine 36, of bromine 78·4, of lead 103·5, of tin 59, &c., notwithstanding that a very high authority doubles several of these numbers.

§ 13. *On the absolute quantity of Electricity associated with the particles or atoms of Matter.*

852. The theory of definite electrolytical or electro-chemical action appears to me to touch immediately upon the *absolute quantity* of electricity or electric power belonging to different bodies. It is impossible, perhaps, to speak on this point without committing oneself beyond what present facts will sustain; and yet it is equally impossible, and perhaps would be impolitic, not to reason upon the subject. Although we know nothing of what an atom is, yet we cannot resist forming some idea of a small particle, which represents it to the mind; and though we are in equal, if not greater,

ignorance of electricity, so as to be unable to say whether it is a particular matter or matters, or mere motion of ordinary matter, or some third kind of power or agent, yet there is an immensity of facts which justify us in believing that the atoms of matter are in some way endowed or associated with electrical powers, to which they owe their most striking qualities, and amongst them their mutual chemical affinity. As soon as we perceive, through the teaching of DALTON, that chemical powers are, however varied the circumstances in which they are exerted, definite for each body, we learn to estimate the relative degree of force which resides in such bodies: and when upon that knowledge comes the fact, that the electricity, which we appear to be capable of loosening from its habitation for a while, and conveying from place to place, *whilst it retains its chemical force*, can be measured out, and, being so measured, is found to be *as definite in its action* as any of *those portions* which, remaining associated with the particles of matter, give them their *chemical relation;* we seem to have found the link which connects the proportion of that we have evolved to the proportion of that belonging to the particles in their natural state.

853. Now it is wonderful to observe how small a quantity of a compound body is decomposed by a certain portion of electricity. Let us, for instance, consider this and a few other points in relation to water. *One grain* of water acidulated to facilitate conduction, will require an electric current to be continued for three minutes and three quarters of time to effect its decomposition, which current must be powerful enough to retain a platina wire $\frac{1}{104}$ of an inch in thickness*, red hot, in the air during the whole time: and if interrupted anywhere by charcoal points, will produce a very brilliant and constant

* I have not stated the length of wire used, because I find by experiment, as would be expected in theory, that it is indifferent. The same quantity of electricity which, passed in a given time, can heat an inch of platina wire of a certain diameter red hot, can also heat a hundred, a thousand, or any length of the same wire to the same degree, provided the cooling circumstances are the same for every part in both cases. This I have proved by the volta-electrometer. I found that whether half an inch or eight inches were retained at one constant temperature of dull redness, equal quantities of water were decomposed in equal times in both cases. When the half-inch was used, only the centre portion of wire was ignited. A fine wire may even be used as a rough but ready regulator of a voltaic current; for if it be made part of the circuit, and the larger wires communicating with it be shifted nearer to or further apart, so as to keep the portion of wire in the circuit sensibly at the same temperature, the current passing through it will be nearly uniform.

star of light. If attention be paid to the instantaneous discharge of electricity of tension, as illustrated in the beautiful experiments of MR. WHEATSTONE*, and to what I have said elsewhere on the relation of common and voltaic electricity (371. 375.), it will not be too much to say, that this necessary quantity of electricity is equal to a very powerful flash of lightning. Yet we have it under perfect command; can evolve, direct, and employ it at pleasure; and when it has performed its full work of electrolyzation, it has only separated the elements of a single grain of water.

854. On the other hand, the relation between the conduction of the electricity and the decomposition of the water is so close, that one cannot take place without the other. If the water is altered only in that small degree which consists in its having the solid instead of the fluid state, the conduction is stopped, and the decomposition is stopped with it. Whether the conduction be considered as depending upon the decomposition, or not (413. 703.), still the relation of the two functions is equally intimate and inseparable.

855. Considering this close and twofold relation, namely, that without decomposition transmission of electricity does not occur; and, that for a given definite quantity of electricity passed, an equally definite and constant quantity of water or other matter is decomposed; considering also that the agent, which is electricity, is simply employed in overcoming electrical powers in the body subjected to its action; it seems a probable, and almost a natural consequence, that the quantity which passes is the *equivalent* of, and therefore equal to, that of the particles separated; i. e. that if the electrical power which holds the elements of a grain of water in combination, or which makes a grain of oxygen and hydrogen in the right proportions unite into water when they are made to combine, could be thrown into the condition of *a current*, it would exactly equal the current required for the separation of that grain of water into its elements again.

856. This view of the subject gives an almost overwhelming idea of the extraordinary quantity or degree of electric power which naturally belongs to the particles of matter; but it is not inconsistent in the slightest degree with the facts which can be brought to bear on this point. To illustrate this I must say a few words on the voltaic pile†.

* Literary Gazette, 1833, March 1 and 8. Philosophical Magazine, 1833, p. 204. L'Institute, 1833, p. 261.

† By the term voltaic pile, I mean such appparatus or arrangement of metals as up to this time have been called so, and which

857. Intending hereafter to apply the results given in this and the preceding series of Researches to a close investigation of the source of electricity in the voltaic instrument, I have refrained from forming any decided opinion on the subject; and without at all meaning to dismiss metallic contact, or the contact of dissimilar substances, being conductors, but not metallic, as if they had nothing to do with the origin of the current, I still am fully of opinion with Davy, that it is at least continued by chemical action, and that the supply constituting the current is almost entirely from that source.

858. Those bodies which, being interposed between the metals of the voltaic pile, render it active, *are all of them electrolytes* (476.); and it cannot but press upon the attention of every one engaged in considering this subject, that in those bodies (so essential to the pile) decomposition and the transmission of a current are so intimately connected, that one cannot happen without the other. This I have shown abundantly in water, and numerous other cases (402. 476.). If, then, a voltaic trough have its extremities connected by a decomposing body, as water, we shall have a continuous current through the apparatus; and whilst it remains in this state may look at the part where the acid is acting upon the plates, and that where the current is acting upon the water, as the reciprocals of each other. In both parts we have the two conditions *inseparable in such bodies as these*, namely, the passing of a current, and decomposition; and this is as true of the cells in the battery as of the water cell; for no voltaic battery has as yet been constructed in which the chemical action is only that of combination; decomposition is always included, and is, I believe, an essential chemical part.

859. But the difference in the two parts of the connected battery, that is, the decomposing or experimental cell, and the acting cells, is simply this. In the former we urge the current through, but it, apparently of necessity, is accompanied by decomposition: in the latter we cause decompositions by ordinary chemical actions, (which are, however, themselves electrical,) and, as a consequence, have the electrical current; and as the decomposition dependent upon the current is definite in the former case, so is the current associated with the decomposition also definite in the latter (862. &c.).

860. Let us apply this in support of what I have surmised respecting the enormous electric power of each particle or atom

contain water, brine, acids, or other aqueous solutions or decomposable substances (476.), between their plates. Other kinds of electric apparatus may be hereafter invented, and I hope to construct some not belonging to the class of instruments discovered by Volta.

of matter (856.). I showed in a former series of these Researches on the relation by measure of common and voltaic electricity, that two wires, one of platina and one of zinc, each one eighteenth of an inch in diameter, placed five sixteenths of an inch apart, and immersed to the depth of five eighths of an inch in acid, consisting of one drop of oil of vitriol and four ounces of distilled water at a temperature of about 60° Fahr., and connected at the other extremities by a copper wire eighteen feet long, and one eighteenth of an inch in thickness, yielded as much electricity in little more than three seconds of time as a Leyden battery charged by thirty turns of a very large and powerful plate electric machine in full action (371.). This quantity, though sufficient if passed at once through the head of a rat or a cat to have killed it, as by a flash of lightning, was evolved by the mutual action of so small a portion of the zinc wire and water in contact with it, that the loss of weight sustained by either would be inappreciable by our most delicate instruments; and as to the water which could be decomposed by that current, it must have been insensible in quantity, for no trace of hydrogen appeared upon the surface of the platina during those three seconds.

861. What an enormous quantity of electricity, therefore, is required for the decomposition of a single grain of water! We have already seen that it must be in quantity sufficient to sustain a platina wire $\frac{1}{104}$ of an inch in thickness, red hot, in contact with the air for three minutes and three quarters (853.), a quantity which is almost infinitely greater than that which could be evolved by the little standard voltaic arrangement to which I have just referred (860. 371.). I have endeavoured to make a comparison by the loss of weight of such a wire in a given time in such an acid, according to a principle and experiment to be almost immediately described (862.); but the proportion is so high, that I am almost afraid to mention it. It would appear that 800,000 such charges of the Leyden battery as I have referred to above, would be necessary to supply electricity sufficient to decompose a single grain of water; or, if I am right, to equal the quantity of electricity which is naturally associated with the elements of that grain of water, endowing them with their mutual chemical affinity.

862. In further proof of this high electric condition of the particles of matter, and the *identity as to quantity, of that belonging to them with that necessary for their separation*, I will describe an experiment of great simplicity but extreme beauty, when viewed in relation to the evolution of an electric current and its decomposing powers.

863. A dilute sulphuric acid, made by adding about one part by measure of oil of vitriol to thirty parts of water, will act energetically upon a piece of plate zinc in its ordinary and simple state; but, as Mr. Sturgeon has shown,* not at all, or scarcely so, if the surface of the metal has in the first instance been amalgamated; yet the amalgamated zinc will act powerfully with platina as an electromotor, hydrogen being evolved on the surface of the latter metal, as the zinc is oxidized and dissolved. The amalgamation is best effected by sprinkling a few drops of mercury upon the surface of the zinc, the latter being moistened with the dilute acid, and rubbing with the fingers so as to extend the liquid metal over the whole of the surface. Any mercury in excess forming liquid drops upon the zinc, should be wiped off.†

864. Two plates of zinc thus amalgamated were dried and accurately weighed; one, which we will call A, weighed 163·1 grains; the other, to be called B, weighed 148.3 grains. They were about five inches long, and 0.4 of an inch wide. An earthenware pneumatic trough was filled with dilute sulphuric acid, of the strength just described, (863.), and a gas jar, also filled with the acid, inverted in it.‡ A plate of platina of nearly the same length, but about three times as wide as the zinc plates, was put up into this jar. The zinc plate A was also introduced into the jar, and brought in contact with the platina, and at the same moment the plate B was put into the acid of the trough, but out of contact with other metallic matter.

865. Strong action immediately occurred in the jar upon the contact of the zinc and platina plates. Hydrogen gas rose from the platina, and was collected in the jar, but no hydrogen or other gas rose from *either* zinc plate. In about ten or twelve minutes, sufficient hydrogen having been collected, the experiment was stopped; during its progress a few small bubbles had appeared upon plate B, but none upon plate A. The plates were washed in distilled water, dried, and reweighed. Plate B weighed 148·3 grains, as before, having lost nothing by the direct chemical action of the acid.

* Recent Experimental Researches, &c., 1830, p. 74, &c.

† The experiment may be made with pure zinc, which, as chemists well know, is but slightly acted upon by dilute sulphuric acid in comparison with ordinary zinc, which during the action is subject to an infinity of voltaic actions. See De La Rive on this subject, Bibliothèque Universelle, 1830, p. 391.

‡ The acid was left during a night with a small piece of amalgamated zinc in it, for the purpose of evolving such air as might be inclined to separate, and bringing the whole into a constant state.

Plate A weighed 154·65 grains, 8·45 grains of it having been oxidized and dissolved during the experiment.

866. The hydrogen gas was next transferred to a water-trough, and measured; it amounted to 12.5 cubic inches, the temperature being 52°, and the barometer 29·2 inches. This quantity, corrected for temperature, pressure, and moisture, becomes 12·15453 cubic inches, of dry hydrogen at mean temperature and pressure; which increased by one half for the oxygen that must have gone to the *anode*, i. e, to the zinc, gives 18·232 cubic inches as the quantity of oxygen and hydrogen evolved from the water decomposed by the electric current. According to the estimate of the weight of the mixed gas before adopted, (791.), this volume is equal to 2·3535544 grains, which therefore is the weight of water decomposed; and this quantity is to 8·45, the quantity of zinc oxidized, as 9 is to 32·31. Now taking 9 as the equivalent number of water, the number 32.5 is given as the equivalent number of zinc; a coincidence sufficiently near to show, what indeed could not but happen, that for an equivalent of zinc oxidized an equivalent of water must be decomposed.*

867 But let us observe *how* the water is decomposed. It is electrolyzed, i. e. is decomposed voltaically, and not in the ordinary manner (as to appearance) of chemical decompositions: for the oxygen appears at the *anode* and the hydrogen at the *cathode* of the decomposing body, and these were in many parts of the experiment above an inch asunder. Again the ordinary chemical affinity was not enough under the circumstances to effect the decomposition of the water, as was abundantly proved by the inaction on plate B; the voltaic current was essential. And to prevent any idea that the chemical affinity was almost sufficient to decompose the water, and that a smaller current of electricity might, under the circumstances, cause the hydrogen to pass to the *cathode*, I need only refer to the results which I have given (807. 813.) to show that the chemical action at the electrodes has not the slightest influence over the *quantities* of water or other substances decomposed between them, but that they are entirely dependent upon the quantity of electricity which passes.

868. What, then, follows as a necessary consequence of the whole experiment? Why, this: that the chemical action upon 32·31 parts, or one equivalent of zinc, in this simple voltaic circle, was able to evolve such quantity of electricity in the form of a current as, passing through water, should decompose

* The experiment was repeated several times with the same results.

9 parts, or one equivalent of that substance: and, considering the definite relations of electricity as developed in the preceding parts of the present paper, the results prove that the quantity of electricity which, being naturally associated with the particles of matter, gives them their combining power, is able, when thrown into a current, to separate those particles from their state of combination; or, in other words, that *the electricity which decomposes, and that which is evolved by the decomposition of, a certain quantity of matter, are alike.*

869. The harmony which this theory of the definite evolution and the equivalent definite action of electricity introduces into the associated theories of definite proportions and electrochemical affinity, is very great. According to it, the equivalent weights of bodies are simply those quantities of them which contain equal quantities of electricity, or have naturally equal electric powers; it being the ELECTRICITY which *determines* the equivalent number, *because* it determines the combining force. Or, if we adopt the atomic theory or phraseology, then the atoms of bodies which are equivalents to each other in their ordinary chemical action, have equal quantities of electricity naturally associated with them. But I must confess I am jealous of the term *atom*; for though it is very easy to talk of atoms, it is very difficult to form a clear idea of their nature, especially when compound bodies are under consideration.

870. I cannot refrain from recalling here the beautiful idea put forth, I believe, by BERZELIUS (703.) in his development of his views of the electro-chemical theory of affinity, that the heat and light evolved during cases of powerful combination are the consequence of the electric discharge which is at the moment taking place. The idea is in perfect accordance with the view I have taken of the *quantity* of electricity associated with the particles of matter.

871. In this exposition of the law of the definite action of electricity, and its corresponding definite proportion in the particles of bodies, I do not pretend to have brought, as yet, every case of chemical or electro-chemical action under its dominion. There are numerous considerations of a theoretical nature, especially respecting the compound particles of matter and the resulting electrical forces which they ought to possess, which I hope will gradually receive their development; and there are numerous experimental cases, as, for instance, those of compounds formed by weak affinities, the simultaneous decomposition of water and salts, &c., which still require investigation. But whatever the results on these and numerous

other points may be, I do not believe that the facts which I have advanced, or even the general laws deduced from them, will suffer any serious change ; and they are of sufficient importance to justify their publication, even though much may remain imperfect or undone. Indeed, it is the great beauty of our science, CHEMISTRY, that advancement in it, whether in a degree great or small, instead of exhausting the subjects of research, opens the doors to further and more abundant knowledge, overflowing with beauty and utility to those who will be at the easy personal pains of undertaking its experimental investigation.

872. The definite production of electricity (868.) in association with its definite action proves, I think, that the current of electricity in the voltaic pile is sustained by chemical decomposition, or rather by chemical action, and not by contact only. But here, as elsewhere (857.), I beg to reserve my opinion as to the real action of contact, not having yet been able to make up my mind as to its being either an exciting cause of the current, or merely necessary to allow of the conduction of electricity, otherwise generated, from one metal to the other.

873. But admitting that chemical action is the source of electricity, what an infinitely small fraction of that which is active do we obtain and employ in our voltaic batteries ! Zinc and platina wires, one eighteenth of an inch in diameter and about half an inch long, dipped into dilute sulphuric acid, so weak that it is not sensibly sour to the tongue, or scarcely to our most delicate test papers, will evolve more electricity in one twentieth of a minute (860.) than any man would willingly allow to pass through his body at once. The chemical action of a grain of water upon four grains of zinc can evolve electricity equal in quantity to that of a powerful thunderstorm (868. 861.). Nor is it merely true that the quantity is active ; it can be directed and made to perform its full equivalent duty (867. &c.). Is there not, then, great reason to hope and believe that, by a closer *experimental* investigation of the principles which govern the development and action of this subtile agent, we shall be able to increase the power of our batteries, or invent new instruments which shall a thousandfold surpass in energy those which we at present possess.

874. Here for a while I must leave the consideration of the *definite chemical action of electricity.* But before I dismiss this series of experimental Researches, I would call to mind that, in a former series, I showed the current of electricity was also *definite in its magnetic action* (366. 367. 376. 377.); and, though this result was not pursued to any extent, I have

no doubt that the success which has attended the development of the chemical effects is not more than would accompany an investigation of the magnetic phenomena.

Royal Institution,
December 31*st*, 1833.

LX. *Remarks on the preceding Paper, with Experiments.*

It has already been shown in the First Number of these "Annals," that Dr. Faraday's labours in attempting to establish a "Relation by Measure of Common and Voltaic Electricity," have been decidedly unsuccessful; and that his attempts to reconcile the deflections of a magnetic needle and electro-decompositions to one and the same law, has proved equally fruitless. The futility of this latter idea would become very obvious by comparing the results of one solitary experiment with some of those which Dr. Faraday has himself given.

Experiment. Let the conducting wires of a voltaic apparatus terminate with large slips of platina foil in a glass vessel (a glass tumbler will do); and in another part of the circuit let there be properly placed the coil wire of a galvanometer. If, now, the circuit be closed by pouring diluted nitrous acid into the glass holding the platina terminals, until it just touch the two, the needle will be deflected, and, after a few oscillations, will rest at some steady angle. This being observed, let more of the liquor be poured into the glass, so that a greater extent of the platina terminals be exposed to it, the deflection of the needle will now be greater than before; and, by making a second addition to the acid solution, another increase in the angle of deflection will be observed: and, by gradually pouring in more and more of the solution until the whole of the terminals be covered, corresponding increases of deflection will be shown by the needle.

This experiment clearly proves that the deflections of the needle are augmented as some function of the transverse section of the liquid conductor; and if electro-decompositions were governed by the same law, we ought to have the latter action, like the former, increasing in some proportion to the platina surfaces exposed to the fluid undergoing decomposition. This, however, is not the case, and is contrary to all experience. It has long been known that much increase in the extent of surface of the platina terminals in a decomposing apparatus, is not attended with advantage; and if any further corroboration of this fact were necessary, Dr. Faraday's

experiments, already alluded to, might very conveniently be adduced. (See paragraphs 714, 722.) Moreover, it may be shown, by employing a feeble electric current, that decomposition can be accomplished by employing small terminal metallic surfaces in saline liquids, and that all chemical action will disappear when large surfaces are employed. But the deflections of a magnetic needle by the influence of these currents, would be in the reverse order. These simple incontrovertible facts are very far from being favourable to Dr. Faraday's hypothesis, and would at any time have a considerable tendency to sap the very foundation of any theory attempted to be erected upon the supposition of these two classes of electro-dynamic productions being governed by the same law.

There can be no room for hesitancy, we imagine, to admit, that, when, in the already-described experiment, the needle became more deflected by an increase in the transverse dimensions of the acidulated liquid in the glass vessel, there was also an increase of electric transmission *through* that section, and consequently through every part of the circuit. This fact, then, compared with the *constancy* of decomposition with different-sized terminals, shows pretty clearly that only a *part* of the current is occupied in the decomposing process, whilst all the remaining portion of the current traverses the liquid as a mere conductor. Hence, so far, it is obvious that even the deflections of the magnetic needle are much superior to electro-decompositions as indicators of the rate of transmission.

Both indications have, however, been indiscriminately employed, and upon the supposed accuracy of the latter class Dr. Faraday has founded the instrument which he calls a voltameter.

It is true that in this instrument the influence arising from variations in the size of the platina terminals can have no effect in modifying an electric current; because when once the size of the terminal metals is determined upon, they will never after vary in their dimensions in the same instrument. This fact, however, would not in the least affect the proof of the inefficiency of the instrument as a measurer of the rate of transmission.

If we compare the results of Dr. Faraday's experiments, as described in paragraph 727, with those which would be shown by a magnetic needle subjected to the influence of all the *variety* of currents that would respectively traverse the corresponding varieties of conducting fluid media which were placed in the instrument, it would appear exceedingly diffi-

cult to reconcile the facts thus exhibited to the inferences which have been drawn, as stated in the *former* part of paragraph 732. We have said the *former* part of that paragraph, because it is from the supposed justness of the statement there made, that the accuracy of the instrument is insisted on. But, (and it is a curious fact) if the paragraph be read through, a very important qualification, amounting to a totally different hypothesis, will readily be perceived. In the former part of the paragraph the instrument is said to measure *all* the "electricity which passed" through it. In the latter part of the same paragraph the instrument is said to measure only *that part* of the "electricity concerned in" the decomposing process. The latter conclusion is precisely that which would be drawn from the experiments already described in these remarks; and since no registry is made of *that part* of the current which *is not* "concerned in" the process of decomposition, the instrument at once becomes disqualified for that important office to which its author has appointed it. The instrument certainly measures the gases liberated by an electric current, especially the hydrogen, when collected separately, and in that capacity becomes an *electro*-gasometer*; but it has no pretensions whatever to the dignified rank of a measurer of either *absolute* or *relative* quantities of the electric matter transmitted. The idea of its indicating the extent of action in the voltaic battery would be perfectly absurd.

Notwithstanding the obvious inaccuracy of the indications afforded by this instrument, Dr. Faraday considers it as "the only *actual measurer* of voltaic electricity which we at present possess," (see paragraph 739); and continues his attempts to vindicate, upon the authority of its indications, that "*the chemical power of a current of electricity is in direct proportion to the absolute quantity which passes*, (783); which proposition in its original form (paragraphs 366, 367, and 377),* obviously applies *ad libitum* to every modification of an electric current; neither its *density* nor its *velocity* being of any consideration whatever. And in paragraph 726 Dr. Faraday has attempted to maintain the justness of this hypothesis by an experiment on which he places a considerable degree of confidence. From a want of information respecting the size of the terminal metals in the different decomposing instruments employed, it would be difficult to form an

* See note, page 95.

* See the "Relation by Measure of Common and Voltaic Electricity." Page, 62.

opinion of the results of either that or any other experiment made in a similar manner; but it may be interesting to know that very different results have been obtained from a series of experiments which seem to be as decisive on this point as any that have yet appeared.

Experiment. Two decomposing instruments were placed in sequence in the circuit of a magnetic-electrical machine;* the terminal metals in the two instruments were platina foil, but of very different dimensions. Those in one apparatus were each one inch high, and a quarter of an inch broad; and those in the other were four inches high, and two inches broad; consequently, the platina surfaces exposed to the acid solution (sulphuric acid and water) in the two decomposing instruments, were as 1 to 32. Both the oxygen and hydrogen gases were collected, from the two pairs of terminals, in the usual way; and the volume from the small terminals was to that from the large ones as 10 to 7. Several trials were made and the difference was never found to be less; but seemed to vary with different velocities of the revolving coils. No error could arise from calculation, because the collecting tubes were of the same dimensions, and the altitudes of the columns of gas had only to be measured, to ascertain their relative proportions. In some previous experiments, copper terminals had been employed in the larger decomposing apparatus, and the hydrogen alone collected. These copper terminals were each about three inches high, and one and a quarter broad. When placed in the same circuit with the small decomposing apparatus, the hydrogen collected in the latter was to that in which the copper terminals were placed, nearly as 2 to 1; certainly never less than as 3 to 2.

Similar experiments have been made with a couronne des tasses of 9 pairs, and the large and small platina terminals already mentioned, the hydrogen alone being collected. The volume from the small terminals was to that from the large ones as 4 to 3.

The experiment was continued for several hours for each of two successive days, and the gas in both tubes carefully collected and measured, once in about every two hours, during the whole time; and the proportions never found to vary in any sensible degree. At the end of the first day's work, the

* By this machine about one cubic inch of the two gases is liberated from acidulated water, every five minutes that it is kept in action. A magnetic-electrical machine having no revolving iron, with a variety of experiments, will be described in our next number, provided we have sufficient room.

gas was well shaken off both large and small terminals, and both portions carefully measured. It was again measured in the morning, and not the slightest loss could be discovered, hence, no absorption had taken place during the night. The experiment was carried on about 18 hours during the two days. A galvanometer coil was placed in the circuit all the time. The deflection fluctuated between 15° and 30°

Similar experiments have been made with more powerful batteries, and with results nearly the same as those last given: but varying a little according to circumstances connected with the circuit. Under some circumstances the proportions were as 3½ to 2; and in other experiments as 9 to 5½. When the water in the small gasometer was mixed with a little sulphuric acid, and that in the large one free from all acid and saline matter, the hydrogen in the former to that in the latter was generally as 20 to 13; and this being the case when the gas collected amounted to many cubic inches, the difference in the two volumes cannot be accounted for by any idea of absorption.

We are not aware that any experiments could be devised which would be likely to show more satisfactorily, than those last described, that the extent of decomposition, by any electric current may be considerably modified by varying the extent of the terminal metallic surface in connexion with the fluid compound; and that even the *same* current, in different parts of the circuit, is productive of different degrees of chemical action *inversely* as some functions of the transverse sections of the fluid part of the circuit undergoing decomposition. But the deflections of the magnetic needle, we may here repeat, are in the reverse order; or, when a fluid conductor (not in the battery) is in the circuit, the deflections are *directly* as some function of the transverse sections of the fluid conductor; the distance between the terminal metals being constant.

It is obvious, moreover, from these facts, that a considerable portion of the electric currents which, between the small terminals, was employed in the decomposing process, traversed the *larger* liquid section as a mere conducting channel, being totally unoccupied as a decomposing agent. From this circumstance it would appear probable that *some* portion of every electric current, traversing a fluid conductor is merely and inefficiently transmitted, without being in any way concerned in the decomposing process. And, as different compounds conduct with different degrees of facility, we have this obvious inference. Different compounds suffering decomposition, transmit *inefficiently* different portions of the electric matter. This inference, however, is decidedly at variance with Dr.

Faraday's views of the subject; who considers that the extent of decomposition is proportional to the quantity of the electric matter transmitted. Nor can we see that it is much more favourable to the idea of electro-decomposition, performed on different compounds, by one and the same current, being proportional to their respective chemical equivalents; a piece of doctrine which Dr. Faraday appears to have been determined on from the commencement of the third series; and which he has attempted to establish by a route not usually pursued by cautious philosophers. We have not room, however, in the present number of the "Annals" to introduce the results of our enquiries on this important point. They may possibly appear in our next.

Several series of experiments will now be placed before the reader which will be found highly corroborative of some inferences already drawn in these remarks. In every series of experiments a galvanometer and gasometer were placed in the circuit, and in some series two of the latter instruments; but with similar terminal metals.

The results expressed in the first, second, and third tables were obtained from a couronne des tasses of ten pairs of copper and zinc, excited by a solution of sulphate of copper.

Table 1, shows every particular at the end of each successive five minutes during the whole period of the first series; and is a specimen of the mode by which the *mean* of each succeeding tabular series was obtained.

TABLE I.

Table of magnetic deflections and chemical decompositions by the same electric currents.

Number of pairs.	Time of action.	Hydrogen gas collected.	Mean deflection of the needle.	Series of experiments.
10	In 1st 5′	4	32°	1st
	2d 5′	3	31·5	
	3d 5′	2	30·5	
	4th 5′	2	28	
	5th 5′	2	27	
	6th 5′	1½	26·5	
	In 30′	14½	29·25°	

From similar series of experiments the following results were obtained.

TABLE II.

Table of magnetic deflections and chemical decompositions by the same electric currents.

Number of pairs.	Time of action.	Hydrogen gas collected.	Mean deflection of the needle.	Series of experiments.
7	30′	10	24°	2nd
9	30′ 30′ 30′	9·5 7·5 7	23·5° 21·5° 20°	3d 4th 5th
5	30′ 30′	2 2	11° 11°	6th 7th
7	30′ 30′ 30′	4 6 6	20° 20° 20°	8th 9th 10th
7 pairs in action but a wire joined the 3d and 8th cups.	30′ 30′ 30′	2 2 1	10·5° 10·5° 10°	11th 12th 13th
4	30′ 30′ 30′ 30′	1·5 1 2 1·25	9·5 9·5 9·5 9·5	14th 15th 16th 17th
9	30′	2·75	13·5°	18th

In the above experiments it is to be understood that the needle never deviated more than 2° on either side of the mean deflection given in the table.

In the two following series of experiments, two electro-gasometers, in sequence, and with similar terminals, were placed in the circuit, and gas collected from one only, and the needle never permitted to vary during the whole time of each series.

TABLE III.

Number of pairs.	Time of action.	Hydrogen gas collected.	Mean deflection of the needle.	Series of experiments.
9	30′	6	20°	19th
	30′	1·25	10°	20th

The next series of experiments were made with a Cruickshank's battery of 50 pairs of 6-inch plates, excited by salt and water; sometimes using the whole, and sometimes only a part of them. One gasometer in the circuit.

TABLE IV.

Large battery of 50 pairs. Salt and water.

Number of pairs.	Time of action.	Hydrogen gas collected.	Mean deflection of the needle.	Series of experiments.
45	30′	12	25·5°	21st
	30′	12	22	22d
50	30′	6	15·9°	23d
	30′	5	14·75°	24th
	30′	5	13·17°	25th
50	120′	16	11°	26th
	rate per 30′	4	11°	27th

The next series of experimental results were obtained from the same battery, the exciting liquid not being removed since the previous day. The action was made a little brisker by the addition of a very dilute sulphuric acid.

TABLE V.

Number of pairs.	Time of action.	Hydrogen gas collected.	Mean deflection of the needle.	Series of experiments.
50	30′	2·5	10·75°	28th
	30′	3	11	29th
	30′	3·5	11	30th
	30′	2·5	10·5	31st
25	30′	1·5	6°	32d
	30′	1·5	6°	33d

TABLE VI.

With two Gasometers in the circuit:—gas measured in one only.

Number of pairs.	Time of action.	Hydrogen gas collected.	Mean deflection of the needle.	Series of experiments.
50	30′	11	22°	34th.
	30′	7·5	17	35th.

The results exhibited in the next table were obtained from a couronne des tasses of nine pairs. The zinc amalgamated, and excited by a very weak solution of sulphate of copper, a wire joined the fourth and seventh pair for the purpose of giving the needle a deflection of 10°, which was kept nearly steady during the whole time.

TABLE VII.

One Gasometer.

Number of pairs.	Time of action.	Hydrogen gas collected.	Mean deflection of the needle.	Series of experiments.
9	30′	1	10	36th.
	30′	1	10	37th.

The experimental results which these tables exhibit require no further comment.

We are well aware of the high estimation in which Dr. Faraday's hypothesis has hitherto been held, but our experimental facts are too inflexible to bend to the transient tide of mere fashionable opinion; and notwithstanding the high scientific authority in its favour, it must be acknowledged by all competent judges, that any series of experiments, however numerous and accurately made, with electric forces of nearly the *same* degree of tension, and from one solitary source of action, are far too limited to afford that variety of data so highly desirable, and even essential and indispensible, in theoretical investigations of this delicate and important character. "In experimental branches of knowledge, authority does not afford conviction, it is destructive if allowed to operate, and in all cases requires scrutiny, careful in propor-

tion as its origin is great*" and, by the same rule, experimental truths ought to be held estimable in proportion as the authority is great which has been arrayed against them.

EDIT.

LXI. *A description of the most powerful electro-magnet yet constructed. By the* REV. N. J. CALLAN, *Professor of Natural Philosophy in the R. C. College, Maynooth.*†

I have lately constructed for the College, an electro-magnet which far surpasses in electric and magnetic power, all the electro-magnets of which I have been able to find a description. The iron bar of our magnet weighs 15 stone; it is $2\frac{1}{4}$ inches in diameter, and more than 13 feet in length. It is bent into the form of a horse-shoe; the distance between the poles is 7 inches. A copper wire $\frac{1}{6}$ of an inch diameter is coiled once round the whole length of the iron bar. This wire is divided into 7 parts, each about 70 feet long. A thin copper wire about $\frac{1}{40}$ of an inch in diameter. is soldered to one of the thick wires at about a foot from one of its extremities. The thin wire is about 10,000 feet long, it is wound round the magnet in the same direction as the thick wire, and in one continuous coil. By connecting the opposite ends of the seven thick wires with the opposite poles of a powerful galvanic battery, an extraordinary magnetic power is communicated to the iron bar; and, by breaking battery communication, an electric current of enormous intensity is excited in the long coil of thin wire. I have tried the magnetic and electric powers of this magnet only once. In consequence of making the trial, in presence of about 300 of the students, I was compelled to omit many of the experiments which I intended to make, and which I expect to make before the end of this month.

In exhibiting the power of the magnet, I first used our large battery of 20 pairs of plates, each 2 feet square, and afterwards, a Wollaston battery containing 280 pairs of 4 inch plates. When the opposite extremities of the 7 thick wires were connected with the opposite ends of the battery of large or of small plates, we found it impossible to separate the keeper from the magnet by any force acting in a direction opposite to that in which the magnetic power was exerted. The keeper was a horse-shoe bar of iron about 20 inches long and $2\frac{1}{4}$ inches diameter. The highest point of the arc formed by the keeper was 7 inches. The distance between its poles was

* Preface to Donovan's Galvanism.

† Communicated by the Author.

the same as the distance between the poles of the electro-magnet. Its weight was about 28 pounds. When the electro-magnet was placed in a horizontal position, and the keeper applied to it, the magnetic power was so great (when the battery of large plates was employed) that the keeper remained without any support, in a horizontal position; and, a weight of about 40 pounds, acting at 7 inches from the poles, or at the highest point of the curve formed by the keeper, was required to turn it out of the horizontal position. The poles of the magnet were so badly ground that a great part of the keeper was at a sensible distance from the magnet.

The magnetic power produced by the 20 large plates was considerably greater than that which was communicated to the iron bar, by the 280 pairs of small plates.

The great electric power of the magnet, or its power of exciting an electric current in the helix at the moment battery connexion is broken, was shown by the most brilliant combustion of charcoal, and by the destruction of animal life.

To one end of the oscillating wire belonging to the electro-magnetic repeater, I tied with a fine metallic wire, a piece of charcoal, in such a way that, on working the repeater, the extremity of the wire and the point of the charcoal should dip simultaneously into the mercury, and should rise simultaneously from it. The opposite ends of the thick wires coiled on the electro-magnet were connected with the opposite ends of our large battery, and the connexion was broken very rapidly by means of the electro-magnetic repeater. As often as the connexion was broken the charcoal and mercury were ignited by the electric current excited in the thick wires coiled round the magnet. The succession of sparks produced by the ignition of charcoal and mercury, was so rapid that they formed one continued blaze of the most vivid light. The combustion of the charcoal and mercury was accompanied by a large quantity of smoke, and was much more brilliant than that which is produced by a voltaic current passed from the battery employed, through a pair of charcoal points.

When, by means of an electro-magnetic repeater, a rapid succession of the electro currents excited in the long coil of thin wire, (at the moment of breaking battery connexion) was passed through charcoal points, they were but slightly ignited. But, although the igniting power of the electric current produced in the long coil of thin wire, was very feeble, its intensity was exceedingly great. For, when it was passed through the body of a large fowl instant death was produced.

I have not as yet examined the decomposing power of our magnet; but, I will shortly try it on some of the simple substances.

I found about four months ago, that an electric current capable of giving a shock, and consequently of producing decomposition, is excited in the helix of an electro-magnet on making, as well as on breaking battery connexion, when the thick wire coiled on the iron bar is short, and when the thin wire is long. Hence it is impossible to obtain separately the elements of bodies decomposed by the electro-magnet. For a similar reason, it appears to me impossible to obtain separately the elements of substances decomposed by the magneto-electric machine. The shock given by the magnetic helix, on *making* battery connexion is weak compared with that which is given, on *breaking* communication. It increases with the number of plates in the battery.

I am now engaged in making an electro-magnetic engine to be worked by our large magnet, or by 26 smaller electro-magnets. Should the engine work well, I expect to send you a description of it, in time for publication in the October number of the Annals.

N. CALLAN.

Maynooth College, June 14, 1836.

LXII. *On a new Insulator for Atmospherical electric apparatus. By* W. Ettrick, Esq.

Sir,

If the method of insulating the wires of atmospherical electrical conductors, suggested by this paper, be approved of by you, I would be much obliged by its insertion in your truly useful Journal, the "Annals of Electricity," a work which has doubtless afforded much instruction to others as well as to myself. As long as the work continues to be carried on with the same spirit as it has hitherto been conducted, I shall contribute my mite towards it.

Your humble servant,
WILLIAM ETTRICK.

High Barns, Sunderland,
June 1st 1837.

It has always been a great desideratum in electricity to obtain a good insulation for the long wires raised for the purpose of collecting the atmospherical electricity. The

difficulty of keeping good the insulation in damp weather was acknowledged by Beccaria and others, but more especially has of late years been adverted to by Mr. Crosse; for we find the following in the London Encyclopædia, article electricity: "Every contrivance was tried to insulate this wire, but Mr. Crosse could not succeed, in preserving the insulation during a dense fog or a driving snow." Much the same was stated by the periodicals last year, in speaking of Mr. Crosse's experiments in electricity, and therefore a perfect insulation is as great a desideratum at the present time as it ever was, and to supply the deficiency the following has been devised.

A B fig. 95, Plate XI, a glass rod 18 inches long, and at least $\frac{3}{4}$ of an inch diameter; 2, 1, 3, 4, a copper or tin cylinder or rather cone, 9 inches long, and $2\frac{1}{2}$ inches diameter at one end, and 4 inches at the other. This cone is furnished with the piece I K M into which is cemented the end A of the glass rod. A ring is fastened at K for securing the apparatus to the support. It must be remarked that this end I M of the cone is closed up and no air allowed to enter. Round the outside of this cone is another similar one, half an inch larger in diameter, C d, E g h f; which is made air tight at both ends, being soldered at C E to the circular end C I M E and at the other end to the circular piece d 2, 4 f, which is made of considerable width in order to prevent the rain water running inwards upon the glass. This cone C d, E f, is furnished with a tube h y, g x, to which a lamp L is adapted, having air holes with covers to keep out the rain at x and y. Directly above this lamp is the bent chimney P O. By this contrivance the flame of the lamp passes round the inside cone 1, 2, 3, 4, and by heating it, warms the air in the inside of it round the glass rod. Lest any rain should blow into the inside of the cone, there is a large cover of tin or copper q R S T u V placed over it, having a cylinder S′ S″ T′ T″ soldered to it for cementing the end B of the glass rod in, and securing the wire to the apparatus. The idea of using heat in this instrument, was suggested during the time I was carrying on some experiments on the conducting power of *Steam* for electricity which I found to be very bad; it immediately occurred to me that by rarifying the damp air in a fog, and warming the glass, a good insulation might be preserved in atmospherical conductors, and the experiments which I have made fully confirm it. One objection to the method naturally suggests itself, namely the difficulty of getting up into a high tree, or mounting to the top of a lofty pole to light the lamp; but this objection is easily answered by another extract from the London Encyclopædia. "A contrivance was adopted to lower the insulators for the

purpose of freeing them from spider's webs." But had it not been possible to lower the insulators, still we would not have been without some method of warming the glass rod. The first which suggests itself is the igniting the combustible body by the electric spark, which would readily be accomplished, if an apparatus was added to the lamp for allowing a small quantity of naphtha, alcohol, or spirit of turpentine, to drop on the wick, by turning a stop cock with a string reaching to the ground. Or the lamp itself might be lowered without removing the other part of the apparatus, and lighted before it is raised. But the best method that has suggested itself to me is to use the gas light when the situation admits of it; for the carburetted hydrogen might be ignited with great facility by the electric discharge, and by turning the stop cock the light instantaneously put out. The expense of carrying a small gas tube up to the apparatus would not be much, and need not be a hindrance to any one. Where gas could not be procured, another method of warming the glass rod might be resorted to, namely, carrying up a metallic tube from a stove heated by charcoal, and might be considered in the light of a chimney to the stove.

LXIII. *An enquiry into the Attributes of the Galvanometer; and how far its indications may be depended upon in Eectro-dynamic Researches.*

(Continued from page 52.)

In the previous part of this enquiry we have been led to conclude, that in consequence of the influence of the numberless circumstances connected with the production of electro-dynamic action, there are no very favourable prospects of arriving at any exact ratios of *permanent* and *transient* deflections from which could be derived much advantage in lessening the labours of enquirers, by the use of tables of *equivalents,* or corresponding deflections, of the two kinds, from different sources of electric action. Our formula *c,* therefore, becomes quite inapplicable to any useful purpose of this kind, since no *substitute* experiments can be depended upon as data, from which could be calculated the extent of electro-dynamic action from other sources.

That part of the enquiry, however, relates only to the deflections of a magnetic needle as admeasurements of the electro-magnetic action of the conducting wire of the coil, simply considered, as a deflecting *force;* without any regard

whatever as to the *nature* of that force; and independently of the character of the elements of which it is composed. The present part of our enquiry will lead us one step farther, and furnish some knowledge respecting the attributes of this celebrated instrument, as a *measurer* of the absolute or *relative* quantities of the electric fluid transmitted through a conducting wire in a given time.

We believe it is generally understood that the electric fluid, to whatever extent it may be accumulated, exhibits no magnetic phenomena whilst in a state of perfect repose: and that (consequently) *some* motion of the fluid in the galvanometer wire, is absolutely essential to produce a deflection of its needle. This being agreed upon, then, if the deflecting force exhibited by any indivdual coil, be proportional to the quantity transmitted in a given time; it will be a matter of no consequence, to the effect produced, whether the *velocity* and *density* be great or small, the indications of the needle would hold good in all cases.

For the number of particles in a transverse sectional film of the fluid will vary as the *area* of the section, and as the *density* of the fluid, whether the current flow equally through the whole interior of the conducting wire, or that it occupies only a certain depth below the surface. Hence if the sectional area be a constant quantity, as it may be supposed to be in a given conducting wire, the number of particles in the sectional film will vary as the *density:* and consequently, the *quantity* of fluid which passes the section in a given time, will vary as its *velocity* and *density* conjointly. If then we denote the sectional area of the current by a, the velocity by v, and the density by d; the quantity q, which flows through the section in the time t'', will vary as $t''\,a\,v\,d$. Or, we may have

$$q = t''\,a\,v\,d$$

which would exhibit the rate of the fluid's transmission through the conductor in all cases. And when t'' and a, are given, as in the supposition, then

$$q = v\,d;$$

which in this case would represent the electro-momentum; and as the ratio of v to d, may vary *ad libitum,* without affecting the equation; it is obvious, that if the deflection of the needle be governed by the electro-momentum, or quantity transmitted in a given time, the varying of the velocity and density would not alter the results: for

$$q = v\,d = n\,v \times \frac{d}{n} = \frac{v}{n} \times n\,d$$

whatever value may be given to n. And by taking the tangent of the angle of permanent deflection, as the value of the electro-dynamic force F, we have

$$tan.\ \Delta = F = q = n\,v \times \frac{d}{n} = v\,d.$$

And, when the density is constant

$$tan.\ \Delta = F = q = v\,;$$

and when the velocity is constant

$$tan\ \Delta = F = q = d.$$

Under these circumstances then, the permanent deflections of the needle might be taken as true indications of the *relative* quantities, of the electric fluid, transmitted in a given time; or the *rate* of transmission from two or more sources of excitation. But as the assumed formula

$$tan.\ \Delta = v\,d$$

rests on no experimental data, other circumstances very naturally present themselves to the mind which tend to put the deductions which this hypothesis would lead to, in a very questionable posture.

Is it known, for instance, that one and the same density will be productive of electro-magnetic action proportional to the velocity in the circuit? A certain velocity will produce a certain effect:—but will twice that velocity give a double deflecting force? or,

$$because\ v \times d = F.$$

$$will\ 2\,v \times d = 2\,F?$$

$$Again,\ will\ 2\,v \times \frac{d}{2} = F?$$

$$will\ also\ \ \frac{v}{2} \times 2\,d = F?$$

The magnetic needle as hitherto used, gives no intelligence whatever in these important enquiries, for the galvanometer in its present form affords no means of ascertaining either the velocities or densities of electric currents; and since the deflections have been considered to depend on these two conditions *conjointly*, without any knowledge whatever of the absolute effects that would be produced by varying either element alone, our ignorance of the extent of the one precludes

our attaining a correct knowledge of that of the other. Thus circumstanced, we should not be warranted in adopting the general formula

$$v\,d = F.$$

in all cases; and for the same reason, any inference to the contrary would be equally unsupported. This profundity of ignorance, however, should not damp the ardour of investigation, for a rich field presents itself in this unexplored ramification of electro-dynamics. This field, however, has not been entirely overlooked nor neglected, for there are on record several experiments which have been considered to bear importantly on this point. These experiments now command our attention, in the examination of which, we shall endeavour to ascertain how far they are susceptible of being enforced as conclusive evidence in support of the hypothesis in question.

The experiments which bear most directly on this point are described by Dr. Ritchie, in the journal of the Royal Institution for October, 1830; and may now be read in continuation of the same paper in which the torsion galvanometer of that philosopher is described. See page 8, of this volume.

Experiment 2. Take two equal rectangular slips of copper and zinc, an inch broad and eight or ten inches long, and divide them into square inches by narrow bands of wax or cement. Solder copper wires to their extremities, and fix them in a small frame, so that they may always be placed at the same distance from each other. Immerse them in a vessel of water, containing a small quantity of sulphuric acid, to the first horizontal division; turn round the torsion key till the untwisting force of the glass thread balances the deflecting power of the electric current, and note the number of degrees of torsion. Immerse them to the second division, turn round the torsion key as before, and the degrees of torsion necessary to balance the deflecting force of the current, from two square inches, will be found double of those for one square inch. Repeat the experiment with three, four, &c., square inches, and the degrees of torsion will be found to be proportional to the surface of the plates immersed.

Having thus shown experimentally the accuracy of the instrument, I shall now apply it to determine the power gained by Dr. Wollaston's contrivance of a galvanic battery above those formerly in use.

Experiment 3. Having provided a clean slip of copper,

two inches broad and about four inches long, I formed it into a rectangle, open at the top, and then covered the inner surface of the bottom with cement. A plate of zinc, of the same size with the rectangle of copper, was placed exactly in the middle, having a face of clean copper opposite each of the sides of zinc. Copper wires being soldered to the rectangle of copper and to the plate of zinc, and their ends dipped into the small metallic cups of the galvanometer, the elementary battery was then immersed in very dilute acid, and the torsion key turned till the deflecting force of the battery was vanquished, the number of degrees being about a thousand. Having removed the battery, I covered one side of the plate of zinc and the opposite surface of copper with cement, and repeated the experiment as before; when, as might naturally be expected, the number of degrees of torsion were found to be very nearly five hundred. We may, therefore, safely conclude that the double plate of copper doubles the *quantity* of electricity without, of course, altering its *tension*.

Immediately after Œrsted's beautiful discovery of the mutual action of magnets and Voltaic conductors, it was known that an immense increase of electro-magnetic power is gained by diminishing the distance between the copper and zinc plates; but, for want of a proper galvanometer, the law does not seem to have been determined with that rigorous accuracy which places its truth beyond the possibility of doubt. To accomplish this was the object of the following experiment.

Experiment 4. In order to avoid every source of inaccuracy, I procured a rectangular wooden box, about a foot long, two inches broad, and two and a half inches deep, into which plates of zinc and copper two inches square might be fixed at any distance from each other. Having filled the box with dilute acid, I placed the copper plate at one extremity and the zinc plate at the distance of *nine* inches, and observed the degree of torsion, as in the preceding experiments. I then untwisted the thread, placed the zinc at the distance of *one* inch from the copper, and observed the degrees of torsion, which were now nearly three times as great as before. This was next repeated with the plates at the distance of nine and four inches, and gave the deflecting forces in the ratio of 2 to 3, which are the square roots of 9 and 4. After trying the effects of the plates at different distances, the following law was established, which had formerly been obtained by a different process: viz.—that the *quantity* of Voltaic electricity circulating along the metallic conductor connecting two plates of dissimilar metals, is inversely as the quare roots of the dis-

tances between the two plates. This law was originally deduced by Professor Cumming,* by observing the deflection of a compass needle, and then taking the deflecting forces as the tangents of the angles of deviation from the original direction of the needle and straight conductor. When I undertook this investigation, it had escaped my memory that any law had been discovered which connected the deflecting force with the distance of the plates. This circumstance, as well as the different process by which it was deduced, affords the most complete proof of its truth.

This law is certainly very different from what we might at first have expected. We might, without experiment, have argued thus: If one inch of fluid between the plates offer a certain resistance to the electric current, two inches will present twice the resistance, three inches three times the resistance, &c. &c. With regard to the cause of this curious law, we can at present scarcely offer a conjecture. Does the electric fluid, after passing through a certain length of an imperfect conductor, acquire some power which enables it to pass more easily through an equal portion? There are phenomena in nature in which imponderable agents do acquire such properties. Light may be so far modified as to pass entirely through glass, which, without such a modification, would have been partly reflected. De Laroche discovered that invisible radiant heat, after passing through a thin plate of glass, passes with less resistance or loss through a second, &c. But, instead of being led away by analogies, which by some may be regarded as fanciful, I shall mention one practical lesson to be deduced from the law in question. In constructing a battery for electro-magnetic purposes, there is not so much power gained as might be supposed by putting the plates very near each other. For

* Whilst looking for an account of these experiments in the Cambridge Philosophical Transactions, we have observed that Professor Cumming had employed Galvanometers with multiplying coils, in many of his very interesting experimental researches, at a much earlier period than the date of the MULTIPLIER of Professor Schweigger. Professor Cumming is therefore entitled to the credit of invention of the first galvanometer of which we have any knowledge.

The Galvanometer of this Philosopher is "described in the Transactions of the Cambridge Phil. Society, for the year 1821, and was invented and its application exemplified by experiment, early in the Spring of that year. A similar instrument was introduced into France, by Œrsted, as the invention of Schweigger, in 1823 (vide An. de Chymie, tom. xxii. p. 358, and Demonferrand Manuel, &c. p. 204): previous to which time it seems to have been unknown, both in France and England." EDIT.

example, if the plates are at the distance of a quarter of an inch, and then at the distance of one-eighth of an inch, the power gained will only be as the square root of 25 to the square root of 125, or nearly as 50 to 35; and the hydrogen constantly escaping, and partially occupying the place of the liquid in the narrow cell, considerably diminishes this apparent increase of power. This circumstance ought not to escape the attention of philosophical instrument-makers in the construction of batteries for electro-magnetic purposes.

Considerable uncertainty still prevails with regard to the law which connects the conducting powers of metallic wires with their lengths. According to professors Barlow and Cumming, the law is the same as that established for fluid conductors.

According to the experiments of M. Becquerel, the conducting powers of metallic wires are simply as their lengths. The following experiment will set the question at rest.

Experiment 5. The galvanometer I have hitherto used requires the following modification for this investigation. Form the rectangle of a single copper wire, and suspend the magnetic needle directly over it, and in the same direction. Take a certain length of the same copper wire, and connect it with a small elementary battery, turn the key, and observe the degree of torsion. Take nine times the length of the same wire, and repeat the experiment with the same battery and acid, and the number and degrees of torsion will only be one-third of those obtained in the first experiment. This experiment I repeated with different lengths of bell-wire, and always found that the intensity of the current was inversely as the square roots of the lengths—the same as the law for liquid imperfect conductors.

M. Becquerel seems to have fallen into the mistake we have now pointed out, by using a galvanometer made of a long wire formed into a coil, and neglecting the resistance the electric current must have experienced in passing through the instrument itself.

The conducting powers of metallic wires, or their ribands, for common electricity, depends almost entirely on their surface, without any reference to their thickness. The fact would seem to be, that common electricity glides along the surface of the metal, being prevented from escaping by the pressure of the ambient air, whereas voltaic electricity requires a certain thickness of metal for its transmission*. Voltaic

* Hence if a metallic rod be raised to a red heat, its power of conducting common electricity is *increased*, whilst its conducting power for voltaic electricity is considerably *diminished*.

electricity, from a single pair of plates, seems to be conducted from molecule to molecule, in some measure resembling the conduction of caloric. Hence, if the diameter of the wire be too fine to allow of this depth of metal, a considerable portion of the electric fluid will be stopped. But, provided the wires be sufficiently thick to allow of this necessary depth of the electric film, then the conducting power ought to be nearly as the circumference of the wire, or as its diameter. If one of the wires be very fine, and the other of a large diameter, this law could not exist. This fact was clearly proved by the following experiment.

Experiment 6. Having taken equal lengths of very fine copper wire and of common bell wire, I used them successively as conductors from the same elementary battery, and ascertained the degrees of torsion as in the former experiments, and found that the large wire conducted better than in the mere ratio of the diameters. For example, the diameter of the one wire was scarcely three times that of the smaller, yet the ratio of their conducting powers was nearly as one to four. I then passed the thick wire through rollers, till it was reduced to a very thin riband,, having its external surface nearly twice that of the original wire, but instead of conducting double the quantity of the original wire, it conducted only three-fourths of that quantity.*

From the law established in the fourth experiment, we need scarcely despair of seeing the electro-magnetic telegraph established for regular communication from one town to another, at a great distance. With a small battery, consisting of two plates an inch square, we can deflect finely suspended needles at the distance of several hundred feet, and consequently a battery of moderate power would act on needles at the distance of a mile, and a battery of *ten* times the power would deflect needles with the same force, at the distance of a *hundred* miles, and one of twenty times the force, at the distance of four hundred miles, provided the law we have established for distances of seventy or eighty feet hold equally with all distances whatever.

Remarks. It will be observed, in experiment 2, that the

* The fact here established bears a striking analogy to a curious fact discovered by Mr. Barlow. He found that it requires a certain thickness of iron or steel to receive the magnetic influence—Is there any relation between the thickness of the iron or steel necessary to receive the magnetic influence and the thickness of the conductor necessary to convey that kind of electricity which acts most powerfully on the needle?

unit for measuring the *quantity* transmitted is the *square-inch* of metallic surface employed in the battery. But it ought to have been known, that the electro-dynamic force of two metallic combinations of the same kind, and extent of surface, very seldom, if ever, give precisely the same angle of deflection: hence if the angle denote the rate of transmission, the battery metallic surface is no true unit by which relative quantities can be measured. But if this be the case with *new* metallic surfaces (and we speak from facts) what shall we say about the much more remarkable error attending the promiscuous employment of *old* surfaces, and *new* surfaces as the measuring unit? The first square inch gave a certain deflecting force:—the first and second, twice that force:—the first, second, and third, gave three times that force, &c.

But we cannot see what information is to be gathered from this method of experimenting. Imagine each part of the experiment to occupy one minute of time, (and it could not be done in much less) the bottom square would be exposed to the acid solution *two* minutes when the second square was immersed *one* minute; three minutes when the third was exposed one minute: and four minutes when the fourth was exposed one minute, &c., and all the succeeding squares from the bottom upwards would be similarly exposed according to their priority of immersion. We hope it is not necessary to inform many of our readers that the electric powers of any pair of voltaic places, or of any given portion of those plates; are very much altered by four minutes' exposure to a solution of sulphuric acid. This fact alone completely disqualifies the experiments for any investigation for which they were intended. And the same objections apply to the 3d and 4th experiments.

We know of no experiments for the admeasurement of *permanent* electro-dynamic forces, better calculated to give just data, than those made by Mr. Barlow,* for ascertaining the force at different distances from the conducting wire. But it must be remembered that this philosopher was not measuring *quantities* of the electric fluid nor the electro-magnetic forces which different quantities would produce. He was not measuring the *elements* of the force, but the *intensity* of the force itself, independently of any consideration whatever as to the extent or character of the elements which entered into its constitution. In these researches Mr. Barlow employed a magnetic needle "only one inch in length," which was never

* See Barlow's magnetic attractions, 2nd edition, part 3.

placed nearer to the conducting wire than four inches;* and as the angle of deflection never exceeded 27°, the poles of the needle, when deflected, were at nearly the same distance from the conductor, in all cases; hence the tangent of the angle was a correct measure of the electro-magnetic force.

A serious error would arise in the computation of electro-magnetic forces by the method of tangents, from data collected by the employment of the common galvanometers, because of the form of the coils and the length of the needles; some of these instruments which we have seen, may answer tolerably well for measuring *transient* electro-magnetic forces upon the principles of pendulous motions, but can have no claim to accuracy as measurers of *permanent* electro-magnetic forces.

(To be resumed.)

LXIV. *Of Communicated Magnetism, or of the various methods of Making Artificial Magnets. From* CAVALLO'S *Treatise on Magnetism.*†

EXPERIMENT I. *To make a piece of soft iron acquire the magnetism from the earth.*

Take a bar of soft iron, about two or three feet long, and between half an inch and two inches thick, (some kitchen pokers are very fit for this experiment) and place it straight up.‡ Then place a magnetic needle on a pin, and, holding the pin in your hand, present the needle to various parts of the bar from top to bottom, and you will find, that in this island, the lower half of the bar is possessed of the north polarity, capable of repelling the north and of attracting the south pole of the needle; and the upper half is possessed of the south polarity, capable of repelling the south and of

* The distances at which the needle was placed from the verticle conductor, were 12, 8, 6 and 4 inches.

† A description of the method of making artificial magnets has long been requested by some of our readers, and we consider it probable that many others will find this article an useful guide.

‡ In a proper manner of making the experiment, the iron bar ought to be placed in the magnetical line, viz. in the direction of the dipping needle; but as few persons are furnished with a dipping needle, and many may be desirous of performing this curious experiment, it will be sufficient for those persons to place the bar straight up, when they are in higher latitudes than forty degrees north or south, but to place it horizontally when they are nearer to the equator than the above mentioned degree of latitude.

attracting the north pole of the needle. The attraction is strongest at the very extremities of the bar, it diminishes as it recedes from them, and vanishes about its middle, where no one pole of the needle is attracted in preference to the other. In short, in that situation, the iron bar is as much a magnet as any piece of iron that stands within the influence of a magnet.*

If you turn the bar top-side down, the extremity of it, which was south pole when it stood uppermost, will now become north pole, and the other extremity will become south pole.

In the southern parts of the world, the lower part of the bar is a south pole; or, to be more explicit, when in any part of the world the bar is situated in the magnetic line, the extremities of the bar will acquire the polarities corresponding to the nearest poles of the earth.

EXPERIMENT II. *To fix in an iron bar the magnetism which is communicated to it by the earth.*

The very soft iron acquires the greatest degree of magnetic power in the shortest time, but loses it with the same quickness; so that if the preceding experiment be performed with a bar of that sort of iron, the magnetism communicated by the earth will not be permanent; but if it be made red-hot, and be left to cool in the magnetic line, or if it be repeatedly struck with a hammer, whilst standing in the magnetic line, it will thereby acquire a small degree of permanent magnetism; which power, however, either by leaving the bar for some time in an improper situation, or by inverting and striking it again, will be soon destroyed.

When the iron is somewhat harder, the acquired magnetism lasts much longer; though a longer time, or longer operation, be required in order to render it magnetic.

As the constant action of a weak magnet on a ferruginous body continually tends to increase the magnetism of that body, so the iron bars which are left in the direction of the magnetic line for a considerable time, become continually more strongly magnetic, and the acquired power becomes more permanent.

The reason why iron, by long standing, by hammering &c., acquires a permanent magnetism from the earth, whereas by the mere position, in a short time, the power is not at all permanent, seems to be the unequal texture of the iron; suppose, for instance, that a piece of iron is composed of hard

* Besides its power on the needle, if the iron bar be not very short, it will even attract small pieces of iron, as filings, &c., that are placed near its extremity.

and soft particles, or of some, through which the magnetic power moves very easily, and others, through which it moves very slowly. The former, then, of those particles acquire the magnetism at first from the earth, and lose it very easily; but by continuing in the same position, or by being softened, &c., the hard particles gradually acquire magnetism from the former, and having once acquired it retains that power for a long time. It is, besides, very probable, and in certain circumstances actually proved, that some sorts of iron become harder by being kept long exposed to the atmosphere.

EXPERIMENT III. *To communicate a permanent magnetism to a ferruginous body, by means of an iron bar, whilst this is rendered magnetic by the earth.*

The simplest method of producing this effect is described in the Philosophical Transactions, by Mr. Arnold Marcel;* part of whose paper I think it not improper to transcribe in this place; it being the first method of the kind that I find published.

"In the year 1726," says he, "making several further observations about the magnetical force which I found in great pieces of iron, I made use of a large iron vice, about 90 pounds weight, in which I fixed a small anvil of about 12 pounds. Upon the bright surface of the anvil I laid the steel, to which I would give the virtue, in a position of north and south, which happened to be in a diagonal of the square surface of the anvil; then I took a piece of iron, one inch square and 33 inches long, of about 8 pounds weight, having one end shaped as at A. fig. 82. Plate XI., and brightly polished; and taper at the other end. Then I held fast down the piece of steel with one hand, and with the other I held the iron bar aforesaid perpendicularly, with its point A. upon the steel, and, pressing hard, I rubbed the steel with the iron bar towards me, from north to south, several strokes, always carrying the bar far enough round about, to begin again at the north, to prevent the drawing back of the magnetical force. Having thus given ten or twelve strokes, I turned the steel upside down, leaving it in the same position as to north and south, and, after rubbing it and turning it, till I rubbed about 400 times, it received by degrees more and more strength, and at last had as much as if it had been touched by a strong loadstone. The place where I began to rub, was always that which pointed to the north when the needle was hung, the end where I had ended the stroke turning to the south. Sometimes it happened, that in a few strokes I gave the steel

* Martyn's Abridgment, vol. vi. part ii. p. 278.

its virtue: nay, even in the very first stroke, one may give a great deal to a small needle. This way I have given the magnetical virtue to needles of sea compasses, made of one piece of steel, so strongly, that one of the poles would take up three quarters, and the other a whole ounce of iron. Although these needles were anointed with linseed oil, which made a hard coat, to keep them from rusting, yet they kept the virtue; but, in strengthening these sort of needles, I rubbed by turns first to the right and then to the left side.

"The same way I brought the virtue into the point of a knife, so that it would sustain 1-$\frac{3}{4}$ ounce.

"I brought the said virtue into four small pieces of steel, each one inch long, and $\frac{1}{12}$ inch broad, as thin as the spring of a watch. These four pieces I joined together, as into an artificial loadstone, weighing 18 grains Troy, and then it did draw up and sustain an iron nail, which weighed 144 grains Troy. This artificial loadstone has now these six years been tumbled about, and been lying among iron and steel, and in any position, and yet has rather got more, than lost any of its virtue."

"The magnetic virtue being thus brought into iron or steel, I have farther observed, that that end where the stroke was begun, would draw to the north, and where the stroke ended, to the south, in whatever situation the steel had been laid upon the anvil to give it the virtue. I took a piece of steel and rubbed it from one end to the middle, and then from the other end to the middle, and found it had two north poles, one at each end, and the middle a south pole, &c.

"Further, beginning to rub from the middle towards each end of another piece of steel, I found it to have at each end a south pole, and in the middle a north pole."

A very easy way of giving magnetism to a small piece of soft steel, is the following: Take two pokers of soft iron, or two iron bars, of about an inch square and more than three feet in length, keep them in the magnetical line, or if in this island, perpendicularly, as shown in fig. 82. Then let the piece of steel C B be either fastened to the edge of a table, or be held by an assistant; and, placing the lower extremity of the bar A B, and the upper extremity of the bar C D, both on the same side, and in the middle of the steel, stroke the steel from the middle towards its extremities, moving the end of the bar C D from the middle of the piece of steel towards its end C, at the same time that the end of the bar A B, is moved from the middle of the piece of the steel to its other extremity B; and when the bars are arrived at the said extremities, remove them from the steel, and apply them

again to the middle, and so on; thus stroking the piece of steel about 40 or 50 times on every side, will give it a considerable degree of magnetism.

It is evident, that if in this experiment, when the iron bars are arrived at the extremities of the steel, you bring them back to the middle of it, by drawing them along the surface of the steel, the experiment will not succeed, because the magnetic power communicated by their rubbing the steel in one direction, will be destroyed by their contrary motion.

EXPERIMENT IV. *To construct artificial magnets after the manner of Mr. Canton.**

Let six bars be made of soft steel, about three inches long, one quarter of an inch broad, and one twentieth of an inch thick. Let, also, six other steel bars be made quite hard, and about six inches long, half an inch broad, and one eighth of an inch thick. Each of those two sets of bars must have two pieces of soft iron called supports or conductors, both equal to one bar of the respective set. One end of each of these twelve bars must be marked with a line, which end is to become a north pole. Have ready an iron poker and tongs that have been long in use.

Place the poker nearly upright, or rather in the magnetical line, with its point downwards; and let one of the soft steel bars be tied, by means of a thread, to the middle of it, and with the marked end downwards; then, with the lower end of the tongs, held also in an upright position, or in the magnetic line, stroke the steel bar from the marked end upwards, about ten times, on both sides, which will give it power enough to keep suspended a small key; thus communicate magnetism to four of the small bars.

This done, lay the two other small bars on a table, parallel to each other, about a quarter of an inch asunder, and between their iron conductors A B, C D, fig. 84, taking care to place the marked end of one of the bars on one side, and the marked end of the other bar on the opposite side. Now place the four bars, already made magnetic, in the manner shown in fig. 85, viz. two with their north poles downwards, and the other two with their south poles downwards. The two of each pair must be placed breadth to breadth, and the two pairs being put contiguous to each other at top, must be kept open at a small angle, by the interposition of some hard substance I. This sort of compound magnet, formed of four bars, must be placed with its aperture on the middle of one of the soft bars A C, taking care to let the south poles H, be

* Phil. Tran. for the years 1751 and 1752.

towards the marked end of the bar A C, and the north poles F towards the other extremity. In this position, the compound magnet must be slid from end to end of the said bar, viz. when the poles H are arrived at C, move the compound magnet backwards the other way, till the poles F come to A, &c. Thus stroke the lying bar four times, ending at the middle; from whence take up the compound magnet, and remove it to the middle of the other lying bar B D, taking care, as above, to let the south poles be towards the marked end of the bar: rub this in the like manner: then turn the bars A C, B D, with the sides which stood towards the table, upwards, and repeat the operation on those other sides. This being done, take up the two bars A C, B D, and let them form the inner two of the compound magnet; and place those which were before the two outside ones, between the pieces of iron or conductors, and rub them with the compound magnet formed out of the other four bars, in the same manner as before. This operation must be repeated till each of the six bars has been rubbed four or five times, by which means they will acquire a considerable degree of magnetic power.

When the small bars have been thus rendered magnetic, in order to communicate the magnetism to the large bars, lay two of them upon the table, between their two conductors or pieces of iron, in the same manner, and with the same precaution, as were used for the small bars; then form a compound magnet with the six small bars, placing three of them with their north poles downwards, and the three others with their south poles downwards. Place those two parcels at an angle, as was done with four of them, the north extremity of one parcel being put contiguous to the south extremity of the other; and with this compound magnet stroke four of the large bars, one after another, about twenty times on each side, by which means they will acquire some magnetic power.

When the four large bars have been so far rendered magnetic, the small bars are laid aside, and the large ones are strengthened by themselves, in the same manner as was done with the small bars.

With some sort of steel, a few strokes are sufficient to impart to them all the power they are capable of retaining; other sorts require a longer operation; and sometimes it is impossible to give them more than just a sensible degree of magnetism.

In order to expedite the operation the bars ought to be fixed in a groove, or between brass pins; otherwise the attraction and friction between the bars will be continually deranging them, when placed between the conductors.

After what has been said in the preceding pages, the reader may easily comprehend the reasons of the operations here described, for constructing artificial magnets.

Experiment, V. *To communicate magnetism by means of two magnetic bars.*

It may be readily understood, that in order to communicate magnetism by means of two magnetic bars, the operation goes on much in the same manner as in the preceding experiment: but, as it is more convenient to use two bars, and as the observations which may be made on the use of them are also applicable to the other methods of communicating magnetism, I thought proper to treat of them apart.

In order to communicate magnetism to a steel bar, to the needle of a compass, &c. place the bar or needle A B, fig. 102, upon a table; then place the two magnetic bars C D, E F, straight up upon A B, at a little and equal distance from the middle of the bar A B, and in such manner, as the south pole D of one of the bars, may be nearest to that end of the bar A B, which is required to become a north pole &c.; then these two bars must be slid gradually towards one end of the bar, keeping them constantly at the same distance from each other; and when one of the magnetic bars, for instance C D, is arrived at A, they must be slid the contrary way, till E F arrives at B; and thus the bar A B must be rubbed a greater or smaller number of times, till it will be found by trial, to have acquired a considerable power. When the magnetic bars are powerful, and the bar A B is of very good steel, and not very large, a dozen of strokes are fully sufficient. When the magnetic bars are to be removed from the bar A B, care must be had to bring them to the same situation where they were first placed, viz. at a little and equal distance from the middle of the bar A B, and then they may be lifted up.

In this operation, the effect of the bars may be improved several ways, which will be found necessary when the bar A B, is proportionately large, and it is required to give it the greatest possible power. This may be effected, first, by joining the magnetic bars at top, interposing a piece of wood or other substance except iron, to keep them apart, as shown in fig. 103; for in this manner, the upper poles of the bars being contiguous, will tend to strengthen each other, and, of course, their lower poles will be strengthened. Secondly, by placing the bar, to be rendered magnetic, between two bars of soft iron, or two other magnets, as shown in fig. 103, or in the manner directed in the preceding experiment. Thirdly, the magnetic bars may be inclined the contrary way, after the

manner used by M. Æpinus, fig. 104, so that the magnets C D, E F, may make an angle of about fifteen degrees with the bar A B.

The bar A B, may, in the same manner, be rendered magnetic, by means of an armed magnet, as shown in fig. 105, or by a horse-shoe magnet, as shown in fig. 106, placing both of the poles of the magnet in contact with the bar, &c.

In all those methods, the bar to be rendered magnetic must be stroked on every side; and in order to let the magnetic centre fall just in its middle, care must be had to stroke one half of the bar just as often as the other half.

Whenever a steel bar, or in general a piece of ferruginous substance, is rendered magnetic by applying two bars, or whenever two magnetic poles are applied to it at the same time, as used in this and the preceding experiment, the operation is usually called the *double touch*, in distinction from the *single touch*, which is when only one magnetic pole is applied to it.

EXPERIMENT VI. *To communicate the magnetic power to crooked steel bars.*

Artificial magnets are frequently made in the shape of a semicircle, or like a horse-shoe, for the sake of bringing both poles in the same plane. These are rendered magnetic in the same manner as the straight bars, excepting only, that the magnetic bars which are used for it must follow the curvature of the steel bar; thus, if it be required to render magnetic the piece of steel A B C, fig. 107, place it flat upon a table, and to its extremities apply the magnets D F, E G, joining their extremities F, G, with the conductor or piece of soft iron F G. Then apply the magnetic bars H, I, to the middle of the piece A B C, and stroke it with them, from end to end, following the direction of the bent steel, so that on one side of it, the magnetic bars may stand in the direction indicated by the dotted representation L K. In this manner, when the piece of steel has been rubbed a sufficient number of times on one side, turn the other side upwards, and repeat the operation till it has acquired a sufficient degree of magnetism.

In this operation, the same precautions must be followed, as were recommended for the method of communicating the magnetism to straight bars, viz., the magnets D F, E G, as well as the magnets H, I, must be placed so that their south poles must be towards that extremity of the bent steel which is required to be made the north pole, and their north poles towards the other extremity. The magnets I, H, must be first placed on the middle of the bent steel, and after having

drawn them over one leg of it as often as over the other, in order to let the magnetic centre fall just in the centre of the bent steel, they are removed, &c.

EXPERIMENT VII. *To communicate magnetism by the application of only one magnetic pole.*

If a person have only one magnetic bar, or a terrella, with which he wishes to give magnetism to a needle or other bar, the only way of effecting it is, to apply one pole of the terrella, or magnetic bar, A B, fig. 108, to one extremity C of the needle and to draw it all along the surface of it till it reaches the other end D, then the magnet being removed, must be applied again to the extremity C, and must be drawn over the needle as before. Thus the needle must be rubbed several times, by which means it will acquire a considerable degree of magnetism.

It must be observed, that the extremity of the needle which the pole touched last, acquires the contrary polarity. Thus, in the present instance, if B be the north pole of the magnet, then the extremity D of the needle will afterwards be found to have acquired the south polarity, and the other extremity C, the north polarity.

In this operation it is evident, that after the first stroke, when the magnet is applied again to C, this extremity, having acquired the north polarity, will have that power destroyed by the vicinity of the north pole B of the magnet; so that it seems that every stroke undoes what was done in the preceding. However, the fact is, that by repeating the strokes the power is increased; but, in general, this method will never be so advantageous as when more than one magnetic pole is used; hence it ought not to be used, excepting in cases of necessity, viz. when one has only one magnetic bar or terrella.

EXPERIMENT VIII. *To show the disadvantages arising from the improper use of magnets of different power, and of steel not properly hardened.*

After having communicated the magnetic power to a steel bar, by means of a given magnet, examine its power; then take a weaker magnet, and with it rub the steel bar again in the same direction as was done before; using the same pole, and in short, following exactly the same operation; after which it might be expected, that the magnetic power of the steel bar was increased; but, on the contrary, it will be found that its power is diminished, being now not stronger than if the steel bar had been rendered magnetic by the second weak magnet alone.

In this experiment, it is required that the second magnet, though weaker than the first, yet be not so strong as to render

the steel magnetic to its saturation, viz, as much as it can hold; for in that case, the difference in the effects of the two magnets could not be observed.

It appears, therefore, that if in communicating magnetism, it be advantageous to use weak magnets first, and then stronger ones, yet the contrary is detrimental.

In respect to the soft nature of the steel that is used for artificial magnets, it must be observed, that soft steel or iron, besides its losing the magnetic power very easily, is subject to acquire more than two poles. This may be observed in the following manner: Take two wires, about fourteen inches long, and an eighth of an inch in diameter; and let one be of steel, quite hard, and the other of very soft steel or of iron, but not of the softest sort; then, by means of magnetic bars render these two wires magnetic, one after the other, treating them both alike; and it will be generally found, that the wire of hard steel will have acquired only two magnetic poles, one at each extremity; whereas the other wire will have more than two poles.

EXPERIMENT IX. *To improve natural magnets.*

The same means by which steel bars are rendered magnetic, or are strengthened in power, may be applied to increase the power of a weak natural magnet, or to render magnetic certain iron ores; but, as the natural magnets are in general very short, one can seldom do more than place them between strong magnetic bars; however, when they are sufficiently long, besides putting them between magnets, they must be rubbed with other magnetic bars, in the same manner, and using the same precautions as were recommended in the methods of making artificial magnets.

For this operation, it is always proper to remove the armature from the natural magnets, if they have any, as is generally the case.

LXV. *Description and Use of an Electro-Magnetic Balance, and of the Pile for constant Currents.* By M. BECQUEREL.*

Until now we have possessed only two means of comparing currents with one another in regard to their intensity: the

* From the "Comptes rendus hebdomadaires des Seances de l'Academie Royale des Sciences," for 1837, No. 2; being an epitome of a paper read before the Academy, 9th January.—Translated by Mr. J. H. LANG.

one is by making a magnetic needle oscillate for a given time, at a certain distance from a wire conductor, traversed by currents not having the same energy, and then calculating the intensity of each of them by means of the formula of the pendulum; the other requires the employment of the multiplier.

By neither of these methods can we reduce the intensities of one current to an easily attainable common measure, an object which should be always keptin view whilst studying the action of forces.

I have endeavoured to compare the electro-magnetic effects of a current by means of weights. The apparatus used for this comparison was arranged as follows :—Take a balance capable of weighing to the fraction of a milligramme; to each of the extremities of the beam suspend from a vertical stem, a scale pan, and a magnet whose north pole is situated in the lower end; afterwards place beneath, on a conveniently situated apparatus, two glass tubes of a sufficient diameter to allow the two bars to enter easily without touching the sides. About each of these tubes is twisted a copper wire covered with silk, so as to form ten thousand circumvolutions. After having placed the bars in the axis of the spirals, make an electric current traverse the wire. Considering first a single spiral, it is very evident that the magnetized bar, as well as the beam with which it is in connexion, will be raised or lowered according to the direction of the current. Let us now dispose of the second spiral, so that the motion of the beam shall be in the same direction when the wire is traversed by the current, and then connect the two spirals; the action that they exercise on the bars will necessarily be increased. Some examples will give an an idea of the use of this apparatus. Having taken two plates—one of zinc, and the other of copper—presenting each a surface of four centimeters square, and in communication with the two spirals, immersing them at the same time in 10 grammes of distilled water, one of the scale pans was weighed down, and it was necessary to add to the other a weight of 2·5 milligrammes to maintain the equilibrium; the magnetic needle of a multiplier, with a short wire, which had been placed in the circuit, was deviated to 60°. By adding to the liquid a drop of sulphuric acid, it was necessary to employ 35·5 milligrammes to maintain the equilibrium: the two currents were then in the proportion of about 1 to 14.

I afterwards sought the relation by weights between currents proceeding from piles composed of more or less numerous elements; with a pile of 40, charged with water con

taining $\frac{1}{60}$ sulphuric acid, $\frac{1}{20}$ marine salt, and some drops of nitric acid, I was obliged to take 615 milligrammes to maintain the equilibrium ; whence it follows that the intensity of this current is to that obtained by a single pair as $17\frac{1}{2}$ to 1.

For measuring thermo-electric currents I made use of spirals like the former, except that they were formed with two series of circumvolutions. I applied it to the determination of the temperatures of the different strata of the flame of an alcohol lamp, by means of two platina wires, not having the same diameter, and united by one of their ends. These temperatures have been found equal to 1310°· 98 ; 913°· 24 ; 743°· 50.

The examples given in my memoir prove with what facility electrical currents of different degrees of tension, may be compared with each other by means of weights.

When we wish to measure the continued action of a force, we must first seek the means of giving it a constant intensity. But the electric current produced by the common piles, and even by a single pair is subject to such continual variations that it is impossible to submit its mode of action to calculation. It is to avoid this inconvenience that we have constructed a pile which excites a current whose intensity does not sensibly vary for 24, and sometimes even for 48 hours.

I published an account some years ago, of an apparatus having the property of producing a current which displays but little variation during the time specified. It was formed of two small glass vessels, one of which contained concentrated nitric acid, and the other a solution of caustic potash, also concentrated. The two vessels communicated with each other by means of a bent glass tube, filled with very fine clay, moistened with a solution of marine salt. In the vessel which contains the alkali immerse a plate of gold, and in the other a plate of platinum. By connecting these two plates with a multiplier, a tolerably energetic current may be perceived, resulting from the reaction of the acid on the sea salt and potash. The gold plate takes negative electricity to the alkali, and the platinum positive to the acid.

To obtain the maximum effect, we must pay attention to the following considerations in the construction of this apparatus. If it were possible to convert into a current, all the electricity which is disengaged by the combination of a given quantity of acid with a proportionate quantity of alkali, this current would, in its turn, be capable of decomposing all the salt formed. Hence, if in the reaction of an acid with an alkali we can secure a sufficiently strong part of the disen-

gaged electricities, we may have a current of an intensity capable of effecting decompositions. To fulfil in part this condition, we take two platina tubes, each bent at one of its ends to enable it to enter into a tube of glass. One of the platina tubes is filled with clay, moistened with nitric acid; the other with clay moistened with a solution of potassa, and the intermediate tube with clay moistened with a solution of sea-salt. The lower extremities of the platina tubes are closed by lids of the same metal, pierced with many small openings. The end of the tube which is filled with the acid-moistened clay is immersed in some nitric acid, and the other in a solution of potassa. To facilitate the transmission of the electricity through the clay to the sides of the tube, a little powdered platinum is mixed with it to increase its conductibility.

Things being thus arranged, to the extremities of the bent branches platina wires are fixed, for the purpose of transmitting the current through those bodies on which we are operating. By uniting several of these apparatus we have a pile whose effects are constant.

One of these pairs alone has required 8·5 milligrammes to maintain the equilibrium. At the same time a short wired galvanometer placed in the circuit gave a deviation of 79 degrees. I have shown in my memoir that the effects of this pile do not sensibly vary for a considerable time.—It is easy to account for this permanency in its effects. We know that the decomposing metallic plates forming part of a voltaic circuit, and immersed in a solution, become polarized, so as to impel a current in a direction opposite to the first: the polarization of each of these plates arises from the deposition of a substance transferred to its surface by the current, and whose nature depends on the position of this plate with regard to the extremities of the pile. While this substance remains in contact with the plate, there will be a current in a direction contrary to the primitive; but if the substance be surrounded by a liquid which has a strong affinity for it, it combines with it, and the plate is immediately depolarized. This is precisely what takes place in the different elements of the pile we have described: the alkali which is transferred to the negative plate combines immediately with the surrounding acid, and that deposited on the positive plate is neutralized by the acid which surrounds it.

I have entered into some details on the electro-chemical effects of polarization of the decomposing plates,

when they transmit uniform currents produced by an apparatus composed of 1, 2, 3, or 4 pairs. I afterwards showed the result of my first experiments with the apparatus already described, for the purpose of establishing the relations which connect the affinities with the electric forces. Since the discoveries of M. Faraday on the definite nature and extension of electro-chemical decomposition, we know that the chemical power of a current is in direct proportion to the absolute quantity of electricity which passes. It is by depending upon this principle that he has determined the equivalents of bodies; but in his researches he has not noticed the absolute intensity of the force which acts at each instant: it is this gap that I have tried to fill up by means of my apparatus. It has been remarked for a long time that the elements which are combined with the most energy are those which are decomposed with the greatest facility by the current, and those which are combined by virtue of the weakest affinities are also those which are least obedient to the decomposing action of the electricity in motion; from which it seems to result, that all compound bodies are decomposed by the influence of a current, in proportion to the force of the affinity which unites their elements. If, then, we could establish a relation between the intensity of the currrent, and the affinity, we should have a means of measuring the latter. In researches of this kind attention should be paid to the following observations of M. Faraday:—1st, that the electric powers, like the chemical action of the electricity are definite; 2d, that a considerable quantity of electricity in the form of a current decomposes only a few elements; 3d, that the electric agent is only employed to overcome the electro-chemical powers—whence it follows that the quantity which passes is at least equal to that which the particles possess when separated; 4th, that there exists a perfect concord between the theory of definite proportions and that of electro-chemical affinity: whence it results that we may consider the equivalent parts of bodies as volumes which contain equal quantities of electricity, or at least which have equal electric powers. The atoms of bodies which are equivalent to each other in their ordinary chemical action, have, therefore, equal quantities of electricity combined with them. The following are the first experiments I made for the solution of the question I had proposed.

When an invariable current is passed through two solutions, at different degrees of saturation, of a salt with a reducible base, the quantity of salt decomposed is precisely the same in both. I took 2·8 grammes of dry nitrate of cop-

per, which was dissolved in 10.3 grammes of water : half the solution was increased by its volume of water ; the two copper wires which were immersed in the two negative branches weighed 0·3385 grammes. After 48 hours' experiment these wires weighed 0·36 grammes : they had gained consequently 0·215 grammes. The intensity of the current which produced this effect was represented by 5 milligrammes.

The intensity being diminished by one-half, the quantity of copper reduced in 48 hours was equal to 0·01 grammes, that is to say, half of that obtained in the preceding experiment.

I submitted the same wire, and the same solutions for 48 hours to the action of a current, counterbalancing 3 milligrammes ; I obtained 0·012 milligrammes of copper ; if, now, the quantities of copper reduced in the two experiments be compared, they will be found exactly proportional to the intensities of the current. Various experiments of the same kind have been made on solutions of nitrate of silver, varying the density of the solution, and the intensity of the current. The quantities of metal reduced were exactly proportional to the variation of the current, the source remaining constant, this being an indispensable condition.

These results follow from the observations of M. Faraday ; but there is this difference between his and mine— viz. That he has not noticed the absolute intensity of the current, whilst I have taken account of it, and we shall see in another memoir the advantages to be derived from the introduction of this new element in experiments on electro-chemical researches.

I have tried, by means of the electro-magnetic balance, when solutions of different metals were submitted to the action of the same current of known intensity, what relation existed between the quantities of metal reduced ; three solutions, one of copper, one of silver, and one of zinc were introduced into the circuit. These solutions were in U tubes, and each of them on the negative side was in contact with a plate of platinum, and on the positive with a plate of the same metal as that in solution ; they were submitted to the action of an apparatus of two pairs prepared with the platina cylinders. The following are the results :—

The intensity of the current counterbalanced a weight of 5·5 milligrammes.

After [24] hours' experiment, the silver precipitated weighed 0·305 milligrammes ; the weight of the copper precipitated 0·090 milligrammes ; that of the zinc precipitated 0·0925 milligrammes. If now we examine the proportion of the three quantities of metal precipitated, we shall find that

they are proportional to the atomic weights of the silver, copper, and zinc; for if we consider the two first, we have 305 : 90 : : 108 (the atomic weight of the silver) to 31·8, instead of 31·6, the atomic weight of the copper. Likewise 305 : 92·5 : : 103, (the atomic weight of the silver) to 32·8 (the atomic weight of the zinc), instead of 32·5, as found by M. Faraday. Hence we see that an apparatus with a constant current, composed only of two pairs, with the electro-magnetic balance, enables us to find the atomic weights of metals, and to determine the quantities of metal reduced, corresponding to a given intensity of current.

LXVI. *Description of Coulomb's Electrical Balance, or Torsion Electrometer. From* Dr. Olinthus Gregory's *translation of Haüy's Natural Philosophy.*

Coulomb has given to the apparatus which he used in the experiments relative to electricity, the name of the *electrical balance;* which is extremely appropriate, since it furnishes the means of establishing the equilibrium between an electric force and another force whose smallest quantities are susceptible of being measured with much precision.

This latter force is denominated the *force of torsion.* It is the effort made by a thread which has been twisted to untwist itself and return to its former state. Let *a c* fig. 109, plate XII. be a thread or wire of metal or other matter to which a small lever *b d* is suspended by its middle; let us suppose that this lever being first in a state of quiescence, begins to turn about a point *c*, by describing circular arcs with its two extremities. The thread becomes twisted at the same time by a number of degrees equal to that which is comprised in each of these arcs: and if we would retain it in this state of torsion, we must apply to one or other of the extremities *b, d,* of the lever, a resistance which will counterbalance the effort of the thread to return to that point where, the lever being immoveable, the torsion would be nothing. Now Coulomb has proved that, all other things being equal, the effort which he names *force of torsion* is proportional to the angle of torsion: let us imagine, for example, that in the case we have been speaking of, the arc described by the point *b*, or by *d*, or which comes to the same, the quantity of torsion was 30°, and let us denote by *r*, the resistance capable of making an equilibrium to that torsion: then, if we suppose a double torsion, or any arc of 60° described, it will be necessary, in order that there should be again an equilibrium, to employ a resistance equivalent to 2 *r*.

The apparatus contrived by Coulomb is composed principally of a glass cage A C D B* fig. 110, covered with a large plate A C, of the same substance. Upon the middle of this plate is fastened a vertical tube *f e b h*, likewise of glass, and surmounted by a much shorter tube or collar of brass *c b h d*, in which another tube of the same metal turns with friction. The latter carries a plate *l y*, having au orifice at its centre to receive a little stem or pivot, on which is fixed an index *o l* that is made to turn at the same time with the pivot. The rim of the plate *l y* is divided into 360 degrees, in the order of *l k y*. The pivot carries at its inferior extremity a little pincer which holds a very thin silver thread *p n*, at whose bottom is suspended a little brass cylinder *n u*, to keep it stretched. This cylinder is split in the direction of its length, and performs the office of pincers to press and retain the little lever *a g*, one of whose arms, namely *n a*, is made of a silk thread varnished with gum lac, and terminated by a little circular plane *a* of gilt paper. The other arm is a copper wire *n g*, of such a length as ensures the horizontal position of the lever. Now, it is in the torsion impressed upon the metallic thread or wire *p n*, that the force consists which serves to measure that of the electric bodies whose effect it balances.

The plate A C has another orifice at *m*, through which passes a second silk thread, varnished also with gum lac, and maintained in the direction *m t*, nearly vertical, by means of a stick *r s* of sealing wax. This silk thread sustains at its inferior extremity a ball *x*, which corresponds to the point *zero* of a graduated circle *z q* fixed, or marked, on the outside of the cage A B C D. We may always, by means of the superior brass tube, which we can turn without difficulty in the collar within which it is fitted, dispose things in such a manner that the little circular plane *a* shall touch the ball *x*, without obliging the thread of suspension *p n* to experience any torsion.†

Things being supposed in this state, we shall proceed to describe the experiments made by Coulomb before the Academy of Science, in 1785. This philosopher first electrified the gilt card *a*, and the brass ball *x*, by touching them with a little conductor charged with vitreous electricity, which he introduced into the cage by an aperture formed for that purpose;

* We may give to this cage either a cylindrical form, such as that represented in the figure, or a cubical form, at pleasure.

† The honourable Mr. Cavandish has employed a nearly similar construction to this of Coulomb, in his valuable experiments relative to the force of gravity. Phil. Trans. 1798.

Or at page 388 vol. XVIII. Hutton's abridgement. Edit.

immediately the ball repelled the little circular plane to a distance of 36°, estimated from the position of that plane with regard to the circumference described upon the glass cage. By a necessary consequence the metallic wire was twisted an equal number of degrees. Coulomb continued the torsion by a farther quantity equal to 126°, by turning the index *o l* attached to the pivot which held the thread suspended; and it will be easy to conceive that, in this case, the rotating motion of the index ought to be in a direction contrary to that in which the gilt circle had moved.

The force of torsion was then found to be considerably augmented, and the repulsive action of the two bodies being no longer sufficient to balance it at such a distance, the gilt circle returned again towards the ball until it reached the point where the repulsive force had so increased in consequence of the diminution of distance that the equilibrium was re-established, at that moment the distance between the bodies was only 18°.

Now it must be remarked that the impressed torsion of 126°, being a continuation of 36° previously produced by the repulsion of the two bodies, if we subtract from this latter the 18° through which the thread had untwisted itself, while the gilt circle returned towards the brass ball, there will remain 18° which, added to the 326 of torsion impressed, will give 144° for the total torsion relatively to the second position of the two bodies. But the torsion which had place in the preceding position was of 36°; whence it resulted that the two repulsive forces which made the equilibrium with these two torsions, were in the ratio of 144 to 36, or that of 4 to 1. Now the corresponding distances were 18 and 36, or as 1 to 2; from which it appears that the repulsive forces conformed to the inverse ratio of the square of the distances.

This experiment was varied in several different ways, according to other relations between the distances; and all the results were found conformable to the same law."

LXVI. *Answer to J.'s. Queries in the last No. of the Annals. By* E. M. CLARKE, *Philosophical Instrument Maker.*

Twist a piece of copper wire round the outside coating of the Leyden jar, A, Fig. 13, connect it with the block of the magnetic electrical machine, (for a description of it see No. 2, page 146 of the Annals). Withdraw the sponge from the director V, and connect its wire with the end of the armature, as in the figure. Rotate the armature at a moderate speed, hold the director by the wood handle, and make it touch the ball for a moment only, as on that depends the success of the experiment, as it is only one spark that shows the fact. Should the director rest on the ball so as that two or more sparks are obtained from the armature you fail. Bring the ball of the Leyden jar in contact with a delicate gold leaf electroscope and the leaves will be diverged. Very little practice will make you perfect in developing their effect. The jar is charged to a very low intensity indeed; but I found that after diverging the gold leaves, if I put my hand on the electroscope so as to discharge it and the gold leaves colapse; on touching the electroscope with the ball of the jar, again the leaves diverged with as much energy as before. I again discharged the electroscope, and again produced a divergence: this I repeated 13 times, with the same effect each time, from the one charge. I had not time to pursue the experiment further, but would be glad to know to what extent it could be carried. The jar I used was 8 inches deep, 5½ inches diameter, open at top—the tinfoil coatings were 6½ inches deep.

Fig. 14, is the form of a conductor, I have always found far superior to any other. I have made a multiplicity of different forms according to the directions of different amateur electricians. I make them of sheet copper and have them japanned very thick, always taking the spark from the brass ball at the end, otherwise you break up the surface of the japan, which causes a partial dissipation of the electricity: the same takes place with lacquered brass. Indeed it is a useless expence having brass conductors; for tinned sheet iron, if japanned, is far better and much cheaper, but copper is best.

FIG. 14.

Your arrangement of the voltaic magnet is decidedly better than the one at the Adelaide Gallery; but still there is an objection to it, namely, spilling your mercury when you break connexion with the voltaic battery, more especially if you have a heavy weight suspended at the time. My plan is—I solder the ends of my wires to a slip of brass, at the end of which is a cylindrical piece having a hole drilled up it. I then have a binding screw, so that when I put my thick conducting wires from the battery into the cylinders, metallic contact is secured by the binding screw.*

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LXVII. *On the application of Electro-magnetism to the moving of Machines. By* M. H. Jacobi, *Teacher of Sciences, and Professor in the Imperial University of Dorpat.*†

PREFACE.

The great discovery of M. Œrsted, which has so much extended the bounds of the physical sciences, promises a new career to the practical mechanic. The agents, which are used at present for the moving of machines, are not, properly called, forces, but only masses animated by forces. These masses are made to act on the point of application of a machine, and consequently they can only give it a speed conformably to their own moving principle. But magnetism permits us to employ a force at once; the point of application is the force itself. We see a very considerable force thus produced without there being any exterior influence. Our interest in such a phenomenon is greatly increased by the simplicity of its production and facility of comprehending it. But upon closer examination we find that those forces which are active in producing motion combine many complicated circumstances. The phenomenology of electricity and magnetism is but confused at present. We are not surprised that almost every day we discover some new phenomena both striking and unexpected. The views that I have taken of these forces have been partly confirmed and partly shaken or even overthrown in the course of my researches; notwithstanding, as soon as I have been obliged to renounce one view another has presented itself, showing signs of favourable results. For example: the

* Dr. Hare keeps his connecting wires together by means of small vices.

† Translated from the French, by Mr. J. H. Lang.

remarkable reaction, which prevents motion from being accelerated to infinity, has become a new source of force; an exact knowledge of galvanic action brings the expenses of keeping it up to a minimum.

I have limited my researches to certain experiments, which very nearly affect the same object, and from them I am going to publish only those which have given the results, or at least make us hope for them. I have as much as possible suppressed all purely theoretical considerations.

The practical application appears to me to be decided by my experiments; to go beyond would only be to enlarge an effect with which one may be already content, as soon as we lay aside our sanguine wishes. It is no great thing to have electro-magnets, which bear 2,000lbs.; mine supports only 30 or 40 at most. Nevertheless, these feeble magnets have given me a mechanical action equal to half the power of a man. To maintain which, for eight hours, every thing being well disposed, barely half a pound of zinc has sufficed.

I have not yet been able to construct a larger apparatus, besides I should wish to work mine to the greatest possible extent, so that it might completely manifest the nature of active forces. My experiments are easily repeated; all that is required is carefully to attend to the construction of the *commutator*, and also that of the galvanic apparatus. Those who are acquainted with electro-magnetic phenomena, will easily make the necessary arrangements, and give the just proportions to the details. The end of this memoir will be attained as soon as it inspires interest for an object which is worthy of it.

Konigsburg, April 21, 1835.

1.

In November, 1834, I had the honour to present a note to the "Academy of Sciences" at Paris, on a new electro-magnetic machine. This note was read at the Session on the 1st, of December, and an abstract printed in the "Institute" No. 82, of December 3d, to which I refer. Since that time M. M. Botto and dal Negro have claimed the priority of the invention, the latter in the Institute, No. 110 of the 17th of June.—The competition in which I was placed with men of such distinguished talent, served but to strengthen my conviction of the utility of this new motive power. A discussion of priority has only an historical interest. It is not surprising that men between whom there is scarcely any communication, should

study the same subject nearly at the same time. But we ought not to deceive ourselves that after the great discovery of M. Œrsted, and the essays of Mr. Sturgeon, who, appears to me to be the first who has given a great magnetic intensity to soft iron by means of an electric current, and has seen the instantaneousness with which this magnetism may be annihilated or reversed, as soon as the electric current is changed; it was not difficult to conceive the idea that some motion or some mechanical work might be produced by the electro-magnetic excitation of soft iron. In reality it is to M. Œrsted that we should grant the palm. We others can only have the merit of knowing how to apply this new force to ingenious works and to the wants of life. This will most likely fall to the lot of him who best understands the mechanical and physical principles of this motive power.

2.

In May, 1834, I constructed the first magnetic machine for continued primitive circular motion. It is true, like M. dal Negro, I had had the idea of applying this force to mechanism for several years, of which I regret not being sufficiently acquainted with the practical part. But I could at first free myself from the idea of making this application, produced by the attractive and repulsive power of magnetic bars, a method of rectilineal motion, which, by means already known, could be converted into a continued circular motion. It appeared to me that a machine of this kind would only deserve to be an amusing plaything, with which physical cabinets might be enriched, but which would be quite incapable of being applied with any advantage on a large scale.

For, considering the general equation of moving forces applied to the motion of machines,

$$\Sigma\int_0^a Mds - \Sigma\int_0^{a'} Pds' = \Sigma mv^2_{,} - \Sigma mv^2_0$$

the magnetic action, during the amplitude a, and represented by $\int_0^a Mds$, would not be perfectly exhausted by the work $\Sigma\int_0^{a'} Pds'$ at least that the moving force gained during the motion, would not have become zero, or $\Sigma mv_{,}^2 - \Sigma mv_0^2 = 0$. But magnetic attraction is a function of that kind of which the form is not yet sufficiently known, this function being affected by the nature of the distribution of magnetism in bodies, whatever may be their form. The law of this

distribution is scarcely established, with relation to bars of steel of a regular form, magnetized to saturation and deprived of its consequent points. As to bars of soft iron of considerable dimensions, magnetized by a spiral electro-conductor, there are no experiments, only analogies. But, whatever it may be, it is well known that this function should be expressed by a very convergent series, so that the magnetic attraction would be in the inverse proportion of the square or cube of the distance ; or, stopping at the three first members, it will perhaps be a compound of them. Then, as the magnetic points approach one another, with an accelerated motion, the moving forces increase and attain their maximum, as soon as a contact is established ; but it is then that this force should be destroyed. It will destroy itself by the fixed points of the machine, and the shocks of the system ; but this will be in an useless manner. There would be in it a real loss of the moving force gained $\Sigma m v_{,}^{2} - \Sigma m v_{0}^{2}$. It is well known how injurious shocks are to the motions of machines ; but in this there is yet another inconvenience, which is not purely mechanical. Soft iron takes by degrees on the surface of contact the nature of steel by these shocks and repeated shakings ; it would have a considerable reacting magnetism, and the transient magnetic force, the only one which serves for motion, would be weakened so much the more. Several experiments made by me on the magnetic force of a bar of soft iron bent to the form of a horse-shoe, and of which I shall speak hereafter, have manifested considerable disadvantages of frequently-repeated shocks, arising from the sudden contact of the armature. But with regard to the mechanical principles of magnetism, it might be objected that the moving force gained would not be absolutely lost for useful work, as a part would be reproduced by the elasticity of the iron itself, and another part might be regained by springs conveniently applied, or by any other mechanical means that might be invented; that I am myself aware of. Let us leave the appreciation of all these fictitious means and digressions to those who are accustomed to construct machines : they well know their insufficiency, the great loss of labour, and the rapidity with which all systems are destroyed if they do not place the greatest care in the preservation of the moving forces. But it is always necessary to look into the nature of the forces for the means of this preservation. The history of the steam-engine informs us that its perfection commenced by James Watt's ingenious idea of stopping the supply of the steam before the piston had finished its course, and making the steam work afterwards by its own expansion. James

Watt had understood the subject; he only gave to the function $P = \Phi(s)$, which expresses the action of the steam, such a form as $\int_0^a Pds = \int_0^{a'} P'ds'$, and thus the active force gained becomes zero, all the injurious and destructive jerks in the machines of the old construction have ceased, and the work of the moving force is for the most part made available for useful purposes. I here certainly ought to quote the researches of M. Poucelet on the construction of hydraulic wheels—a work established on a profound understanding of these principles.

These simple and clear considerations have obliged me entirely to reject all contrivances for the application of magnetism to the direct production of rectilineal motion; these methods being, as we have seen, also inadmissible on account of their impracticability on a large scale.

3.

In the note that I had the honour to present to the Academy of Sciences at Paris, I have advanced, after all my experiments, that magnetism is a force, acting like universal gravitation, purely as some function of the space. The integral $\frac{\int_0^a Mds}{a}$ comparable to the known number g, represents the working power produced by the attraction of two points and is in no way affected by their relative speed. The reversion of the poles being effected instantly, there would thus be an infinitely accelerated velocity. But a system, moveable on an axis and capable of having a continued circular motion is the only one which could be susceptible of such a velocity. It could not become uniform unless some resisting element or some other work depending upon speed, entered the system. Laying aside the available work which ought to be performed by such an apparatus, the obstacles to be overcome, inseparable from the system, consist only in the friction of the pivots, and the resistance of the air—as to the first, repeated experiments have proved that the friction is independent of the angular velocity, at least within the limits of experiment, hence this resistance can contribute nothing towards rendering the accelerated motion uniform. It is in fact the resistance of the air which will produce this effect. Even though it might be reduced at pleasure, principally by giving a convenient form to the gyratory system, it can not be quite annihilated. But we should have reason to be very well contented

with the mechanical effect of magnetism if this were the only cause that reduces the accelerated to an uniform motion. The limits of such an uniform velocity will be very extensive; I do not speak of the great simplicity of a magnetic machine for a continued circular motion, of the advantages that are gained by the construction, it being easy to convert this motion into any other that the productive apparatus may require. I was first struck with these considerations when the means of putting them into execution were yet unknown, but I always had their practical application in view, and the object appeared to me of too great importance, to exhaust my powers in the construction of playthings which could only claim the honour of being put in the rank of electric chimes in relation to their effect, and still more with regard to the noise with which they are accompanied.

4

Fig. 111, Plate XIII., represents the magnetic machine of eight bars symmetrically arranged on a disc, moveable about the axis A, A, and eight bars similarly disposed on a steady frame. The arrangement of the bars admits of the greatest variety, provided that it be exactly symmetrical, and that it permit the poles to approach one another as near as possible. In order that the action be not too oblique since the magnetic centre of gravity is probably at some distance from the extremity as in the ordinary magnetic bars, it would be better to make this arrangement so that the axes of the cylindrical bars may be at right angles, and not parallel as in the figure. It must also be observed that there will be some difficulty in forging the bars of large dimensions into horse-shoes, such that the axes of the legs may be exactly the same distance apart and the legs themselves perfectly cylindrical. In reworking them with the file, there will perhaps be the disadvantage of making the surface of the iron too hard, so as to render it less ready to take and lose the magnetism. The form indicated offers yet some inconvenience as to the application of the spiral of copper wire, which ought to be first formed on another cylinder of the same dimensions. These spirals ought almost to touch the bars, which must be covered with silk to give the necessary insulation. Hence, an arrangement such as is shown in fig. 112, will be preferred, where *f* represents the fixed bars, and *m* the bars moveable about the axis *a*: having the advantage of enabling us to use bars of cylindrical soft iron such as is found of all sizes in the

warehouses—so that the only labour necessary, will be to cut it into equal pieces and the spirals may be forcibly bent round the bars by means of the lathe.

5

Since magnetic attraction decreases rapidly when the distance increases, the integral $\int_0^a Mds$ will be always a certain function of the amplitude a, as its value will not differ much from a constant, a being rather considerable. Admitting for a moment, that the magnetic attraction be in an inverse proposition to the square of the distances, we shall have $\int_0^a Mds = \int_0^a \frac{fds}{d^2+s^2} = \frac{f}{d} \text{arc } tg \frac{a}{d}$, d being the distance of the magnetic centres. The bars are placed as near as possible, so that d being very small with regard to a, $\int_0^a Mds = \frac{f\pi}{4d}$ or $= \frac{nf\pi}{4d}$, n representing the number of bars. Then we shall have for the work of the mover during an entire revolution, the expression $\frac{n^2 f\pi}{4d}$. The radius of the circle on which the bars are arranged does not enter into this expression and for a stronger reason, it will not enter into any other expression if the attraction increase more rapidly than the inverse proportion of the square of the distance. Thus the size of the circle for the same number of magnets adds scarcely anything to the work of the mover.

I have also thought that the system of bars which are fixed in my machine might be rendered moveable also. The rotation of the two systems would then be in contrary directions, and of equal velocities, the masses being equal. These two motions could be reunited by means of conical wheels, so as to produce a rotatory motion on a second axis destined for the work. The work of the mover during the amplitude a, that is to say, from one meeting of the poles to another, would be as before $\frac{nf\pi}{4d}$, but the poles would meet $2n$ times in a revolution: we should have $\frac{2n^2 f\pi}{4d}$ or double of the preceding work. Cog-wheels might even be constructed, so that the velocities :the systems might be in the ratio of m to 1, and that the les might meet $(m+1)\ n$ times during a revolution. The work would then be $(m+1)\ n^2 \frac{f\pi}{4d}$, and we should have gained this additional power by purely geometrical means.

This is deduced simply from the velocity not entering into the magnetic attraction. I have not as yet profited by this arrangement in the construction of magnetic apparatus, since there are some remarkable circumstances (as will be seen hereafter), not yet sufficiently clear, and which will leave room for some considerable modifications.

(To be continued.)

ELECTRICAL SOCIETY OF LONDON.

It gives us great pleasure to learn that Electricity is no longer to be left to the isolated exertions of individuals, or the chance notice of learned societies.

The praiseworthy labours of individuals have been crowned with such splendid discoveries that electrical science has long ceased to be considered as a mere amusing toy. As an universal agent in nature and space, it takes the first rank in the temple of knowledge; but while many other branches of science have possessed the powerful and well-directed aid of associated men and funds, that of Electricity, scarcely second to any in importance and interest, has been left to the fitful energies of individuals. There has been no common centre for the accumulation of facts and data; and thousands of important experiments as well as much acute reasoning have been lost, for want of union among those who have been devoting their attention to the science.

"The business of philosophy" says Priestly, in his History of Electricity, "is so multiplied that all books of general philosophical transactions cannot be purchased by many, nor read by any person. It is high time to *subdivide* the business, that every man may have an opportunity of seeing every thing that relates to his own private pursuit. Let the youngest daughter of science set the example to the rest, and show that she considers herself considerable enough to make her appearance in the world without the company of her sisters." It is much to be regretted that the advice of the first electrician of his age has not hitherto been carried into effect, and it is with this view that the Electrical Society of London has been formed (as expressed in its prospectus) for the purpose of the experimental investigation of Electrical Science.

The Society at present appears to be of the simplest form, consisting of Resident and non-Resident Members, who meet weekly for scientific investigation, discussion &c., and we, of course, need scarcely observe that the formation of a Library, bearing on Electricity, Galvanism, Magnetism, &c., as well

as a collection of suitable apparatus, are among the most prominent of its objects.

The labours of the Astronomer, Geologist, and Chemist, will now have for their aid, a Body ready to pursue with effect and vigour any line of investigation which mutual pursuits may suggest; and there will thus, we trust, be another of those enobling links of brotherhood amongst mankind of all nations, drawing still closer the relationship of intellect.

We cannot forbear expressing our earnest wish for the success of this undertaking: indeed we consider it is destined to hold a place among the first learned Societies of our country. In the mean time, following up the promises made in the Prospectus of these Annals, of presenting *to the Reader every novel fact as it becomes known in this department of Science,* we cannot better further it objects, than by thus announcing the existence of a Society so peculiarly formed to carry those objects into execution.

The brevity of its rules enables us to lay them before our readers. The object is so plainly defined, that any comment of ours becomes totally unnecessary. The Society has our best wishes, and as far as our humble efforts can be made available, we shall at all times be happy to render it any assistance in our power.

The Electrical Society of London

Is established for the purpose of the experimental investigation of Electrical Science, in all its various branches, in order not only as far as lies in the power of the Members to advance that important Science, by entering upon, and pursuing original paths of investigation, but also to examine the experiments of other enquirers.

That their labours may be the more effective, the Provisional Committee invite the aid of all those who are engaged in similar pursuits, either by their becoming Members, or in any way which they consider may tend to promote the interests or further the views of the Society.

RULES.

1. The Society, for the present, to consist of Resident and non-Resident Members.
2. All Subscriptions to commence on the 16th of May; that of a Resident Member (or one residing within 20 miles of London) to be for the first year *Two Guineas,* and that of a non-Resident *One Guinea.*
3. Visitors may be introduced by any of the Members and will be permitted to join in the discussions.

4. When 50 names have been registered as Members, a general meeting will be called, and a Council appointed, to prepare the standing Rules and Regulations of the Society. Such Rules to be proposed at one, and confirmed or rejected at a subsequent general meeting.

5. The Society to meet every Saturday Evening, at seven o'clock, for the purpose of conversation, and at eight o'clock the Chair to be taken. The Chairman being elected by the Members present.

6. At the first meeting in every month, and after the immediate business is disposed of, it shall be competent for any Member to propose such motion as he may consider advisable for the interests of the Society, notice of which, however, must have been entered in the minutes of a previous meeting.

ORDER OF BUSINESS.

Immediately on the Chair being taken, the minutes of the previous meeting are to be read, and the names of any Members subsequently registered, announced. The Secretary shall then read such papers as have been presented to the Society during the past week. After each paper has been read, a discussion may take place, should the Members present consider it expedient.

SECRETARY.

The duty of the Secretary (*pro tem.*) will be to keep minutes of the meetings of the Society, and to enter them in a book, to which any Member may have access; to register the names of Members and Visitors, and to take charge of all communications made to the Society.

TREASURER.

The duty of the Treasurer (*pro tem.*) will be to keep the accounts of the Society; to receive the Subscriptions, and to enter the same in a book, which will be open to the inspeetion of the Members.

All donations of money, books, or apparatus, to be entered in the minutes of the next meeting after they have been received, when they will be duly acknowledged.

Every paper communicated to the Society, in order to be read at its meetings, will be deemed the property of the Society, unless any engagement to the contrary shall have been stipulated by the Author.

Any Gentleman wishing to become a Member is requested to forward his name to

THOMAS PATRICK, *Honorary Secretary*,
No. 11, Lowther Arcade, Strand.

Some very excellent papers have already been read at the weekly meetings of the Electrical Society: and animated and highly scientific discussions have taken place by some of the members present.

Amongst the papers read, we may mention the following—

1st. An attempt to explain the Phenomena of Heat, Electricity, Galvanism, Magnetism, and Light, Illustrated by many diagrams, By T. Pollock, Esq.

Mr. Pollock presented to the Society, a treatise on this subject.

2nd. On a New Torsion Galvanometer, with two threads. By M. Roberts, Esq.

3d. On a Metallic Rope conductor for Ships. By M. Roberts, Esq. The rope is made of thin copper wire, and is as flexible as a hempen one. A specimen was shown to the Society.

5th. On Voltaic Combinations, Illustrated by several experiments. By the Rev. John Shillibeer, M.A. Mr. Shillibeer's battery is described in No. 3, of the Annals of Electricity. Twelve of these were arranged in series, on the lecture table; by which water was decomposed with very great rapidity. Charcoal points were ignited; and other phenomena exhibited by this powerful miniature battery. Mr. Shillibeer passed some high encomiums on the motives and prospects of the Electrical Society; a Society which has long been wanted, and from which many valuable accessions to Electricity and its collateral sciences may be expected. Mr. Shillibeer wished every success to the Society, and to further its views very liberally presented to it, twelve of his very neat and excellent batteries.

6th. Mr. Sturgeon read a paper on Secondary Electric Currents, Illustrated by a powerful Horse-shoe Electro-magnet, which had been presented to him by Professor Callan, of the R. C. College, Maynooth.

The magnet was brought into play by a single voltaic pair, placed in a pint jar. Some of the members and visitors experienced very forcible shocks from this magnet.

The proceedings of the Electrical Society of London will appear regularly in these "Annals," and every interesting paper given entire.

THE ANNALS

OF

ELECTRICITY, MAGNETISM, AND CHEMISTRY;

AND

Guardian of Experimental Science.

OCTOBER, 1837.

LXIX. *On the application of Electro-magnetism to the moving of Machines. By* M. H. Jacobi, *Teacher of Sciences, and Professor in the Imperial University of Dorpat. From the "Comptes rendus hebdomadaires des Seances de l'Academie Royale des Sciences."**

(*Continued from page* 415.)

6.

The reversion of the poles is an object of the greatest importance. This change should operate instantly, and exactly when the poles are opposite one another. The mechanism destined to produce this operation ought to be put in motion by the machine itself; but no element should enter there, which might not be geometrically dependant upon the gyratory motion of the system. The velocity of the motion, however great it may be, ought not in the least to affect this operation. The well known bascule, an ingenious invention of M. Ampère, which is used with so much advantage in electro-magnetic experiments, cannot serve for a magnetic apparatus of continued circular motion, for the number of changes in a given time could scarcely be very considerable without requiring extraordinary means. And even these means would not guarantee the certain result of a rectilinear motion repeated as often as would be necessary. I shall not here mention all the attempts, both numerous and expensive, that I have made to attain the important result of an exact and rigorous reversion of the poles, independent of any element arising from the velocity. But it is proper to state that the greatest difficulties were caused by employing mercury, as was customary in electro-magnetic experiments, to establish and break the metallic contact. In the liquid state it is the adhesion of the mercury to the metallic body immersed in it, and afterwards

* Translated from the French, by Mr. J. H. Lang.

drawn out, which varies with the velocity of the motion and the purity of the mercury. Often, I may say always, the reversion takes place too soon or too late, and thereby causes an attraction or repulsion, in a direction contrary to the rotation. Besides, it is very difficult to preserve the purity of the mercury, when it is in contact with other metals ; and even the purest mercury is easily disposed to oxidize under the influence of electric sparks. These sparks are produced, when the circumstances are favourable, in establishing the metallic contact, and always in breaking it. Hence the reason that the mercury is soon covered on the surface with an oxydized stratum, which totally hinders the metallic contact, or at least renders it imperfect. By employing amalgamated surfaces, this effect is produced still sooner. Besides, I am perfectly convinced by incontestible proofs, that the simple contact of metals with a clean surface, suffices to conduct entirely the electric current, even of the weakest tension. The contact, by the interposition of the mercury, adds nothing to the energy of the current. It is an error to judge of this energy by the brightness of the spark, arising only from the combustion of the mercury. It was proper to mention these seemingly unimportant circumstances. In a mover by which it is pretended to gain an infinitely accelerated motion, there are rarely any circumstances that may be rejected; in short the least of them are valuable.

7

Fig. 113 represents the commutator, adapted to the magnetic apparatus, to produce the reversion of the poles. *a, b, c, d* are four copper discs, fixed on the axis of rotation *e e*. The discs *a b* and *c d*, are united by copper tubes *f,f,* and perfectly insulated from the axis by the interposition of a tube *g* of varnished wood, or any other insulating substance. The periphery of each disc is divided into eight equal parts, of which four *h* are cut into sectors and afterwards filled with pieces of ebony, forming a smooth and even surface with the metal. The discs are arranged on the axis of rotation, so that the sectors of wood and metal correspond alternately, as represented by the shaded parts of the figure. *z z, c c,* are copper bars, in the form of a lever, very moveable in their supports. They are intended to conduct the current. The long arm of the lever forms at its extremity a ridge, which rests on the periphery of the corresponding disc. The other arm is bent and placed in a little cup filled with mercury, *k*. As it is represented by fig. 111, the cups *k k*, and *k' k'*, are united by the copper strips. The action of this commutator is easily perceived. The levers are always in contact with the discs, alternately

with the metallic and insulating parts. By their flexibility in their supports they yield to the least inequality in the surface, and the friction they occasion is very trifling. The spirals which surround the moveable bars are connected so as to form one continued wire, of which the ends *l*, *m* are respectively soldered to the systems of the discs *a b*, and *c d*. The other spirals, bent round the fixed bars, are also connected, and the ends *n* and *o* dipped, one in the cup of mercury *p*, attached to the voltaic apparatus, and the other in the cup *k* of the commutator; so that by the intervention of the commutator the whole of the sixteen spirals form but one wire. The voltaic apparatus consists of four copper troughs, in which are placed zinc plates, all being united in a pile. The direction of the current is represented in fig. 113 by small arrows; it is reversed every time the poles meet, provided the commutator be so disposed that the ridges of the levers quit one of the divisions to pass to the other. This change, as will be seen, operates instantly, and quite independent of the velocity of rotation. The subject is too simple, and too well explained by the figures, to render it necessary to enter further into its details. I may add, further, that this system for changing the poles applies to any number of bars, provided that the sections of the discs are equal to them in number. I have constructed for magnetic electrical experiments, a double commutator of eight discs, divided into seventy-two sections. In this apparatus are four levers, similar to the former, which rest on the cylinders (*f*) that connect the discs in pairs. The other extremities of these levers also dip into cups of mercury intended to receive the ends of some connecting wire, which is to be traversed by the magnetic, electrical, or voltaic currents, sometimes in one direction and sometimes in another. The instrument is set in motion by a winch, which may easily be turned twice in a second, effecting in the same time 144 double reversions. It will be easy to change or entirely stop the electric current 1000 times in a second, and even more. We may without doubt enter more intimately into the nature of this current, or magnetism, by decomposing it into a series of very rapid pulsations. I am persuaded, that we may be able by this means to charge a Leyden jar, or effect any chemical decompositions whatever, by the thermo-electric current of a single pair.

8

It is well known that magnetic force is excited and retained by the action of the voltaic apparatus. In making use of zinc as a positive metal, copper as a negative, and diluted sul-

phuric acid as a liquid conductor, the transformation of the metallic zinc into sulphate of zinc is what constitutes the expense of keeping it in action. It is of great importance to reduce these expenses as much as possible. Let us examine what relation there is between the magnetism of the connecting wire and the action of the voltaic apparatus. Since the discovery of elctro-magnetism, several scientific men have been engaged in this subject, but it offers so many difficulties, and such a complication of circumstances, that we ought not to wonder at the considerable differences in the formulæ and theories they have tried to deduce from various experiments. This is not the place to enter into criticism on these theories; but it appears to me that the theory established by M. Ohm, in a little work, entitled "*Die galvanische Kette, mathematisch bearbeitet von Dr. G. S. Ohm* (1827)." and more fully developed in various memoirs printed in the German Journals, offers so much simplicity, and agrees so well with all the phenomena of the voltaic pile, that I have not hesitated to adopt it, in order to gain from it a general base for the arrangements of the different elements of the magnetic apparatus. I shall here be permitted to announce the fundamental principles of this theory.

1. In a closed voltaic circuit the same quantity of electricity passes over each section that is perpendicular to the direction of the current, whatever may be the form or matter of the different parts that form the circuit.

2. Whatever change is made in one part of the circuit, affects the whole action of the pile, and does not terminate in the same place as the change took place.

3. In whatever manner the voltaic action is measured it will be found to be in the direct ratio of the electro-motive force and inversely as the resistances which are opposed to the passage of the current, or $A = \frac{E}{R}$

4. The resistances are composed

a.) Of the resistance of the solid conductor, or of the connecting wire. For the same substance, this resistance is in the direct proportion of the length of the wire, and inversely as its transverse section or thickness.

b.) Of the resistance of the liquid conductor; this is in direct proportion to the thickness of the stratum which separates the positive plates from the negative, and inversely as the transverse section which coincides, for the most part, with the surface of the plates.

During the action of the pile, this latter resistance increases

and at the same time the electro-motive power, or E, is affected by it. This is because some chemical effects take place, which change by degrees the nature of the liquids, the surface of the metals, and the electric tension. But taking any state of the pile, the law quoted, always exists. The difficulty of comparing electro-magnetic experiments together, and the still greater difficulty in arriving at the absolute measures, consists principally in the continual change of these elements. Thus in expressing by r the resistance of the connecting wire, we shall have $\frac{rl}{d}$ for the resistance of a wire, of a length and thickness d; $\frac{r'\,l'}{d'}$ will in the same manner be the resistance of the liquid conductor, whose surface and thickness are respectively expressed by d' and l'. Therefore, the action of the current or the quantity of the electricity passing through the pile will be $A = \dfrac{E}{\dfrac{r\,l}{d} + \dfrac{r'\,l'}{d'}}$

5. The electro-motive force is in direct proportion to the number of voltaic pairs united in a pile, and at the same time the resistance r' increases in the same proportion. Having a pile of n' pairs, the force of the current will be expressed by

$$A = \frac{n'\,E}{\dfrac{r\,l}{d} + \dfrac{n'\,r'\,l'}{d'}}$$

6. If the electric current be divided into several branches, whose lengths, reduced in the inverse proportion of their diameter, are expressed by l, l', l'', &c.; the total action will be the same as if it had only one connecting wire, whose length is expressed by the equation $\frac{1}{L} = \frac{1}{l} + \frac{1}{l'} + \frac{1}{l''}$ &c.

Then having n wires of the same length, the total force of the current will be expressed by

$$A = \frac{n'\,E}{\dfrac{r\,l}{nd} + \dfrac{r'\,l'\,n'}{d'}} = \frac{n\,n'\,d\,d'\,E}{r\,l\,d' + r'\,l'\,d\,n\,n'}$$

As we can profit by the magnetizing power of each unity of length of the connecting wire, by bending it about bars of the same dimensions, the total force gained by a connecting wire l, will be

$$A = \frac{l\, n\, n'\, d\, d'\, E}{r\, l\, d' + r'\, l'\, d\, n\, n'}$$

From this formula may be deduced the limits of the action of the current, which cannot be augmented by the number or the surface of the voltaic pairs, by the length, diameter, and number of the connecting branches.

Increasing only the surface of the pairs d', the limit of the total force of the current will be $A = \frac{n\, n'\, d\, E}{r}$ by increasing the number n$'$, this limit is $A = \frac{l\, d'\, E}{r'\, l'}$

In the same manner this limit will be, by increasing

$$\text{the length of the wire, } l,\ A = \frac{n\, n'\, d\, E}{r},$$

$$\text{the thickness of the wire d, } A = \frac{l'\, d'\, E}{r'\, l'}$$

$$\text{the number of the branches n, } A = \frac{l\, d'\, E}{r'\, l'}$$

Generally, to increase the force of the current as much as may be required, it will be necessary to increase the surface of the pairs, and at the same time the thickness of the connecting wire or the number of branches.

The increase of the number of the pairs requires that of the length of the connecting wire also, to attain the same end.

The experiments, as exact as numerous, that M. Fechner has made on this subject, and published in his work "*Maassbestimmungen uber die galvanische Kette* (1831)" leave no doubt as to the truth of these laws, which express in a very simple manner all the relations of the different elements, which constitute the voltaic pile. These experiments have been made for the most part by means of the method of oscillations, which M. Biot was the first ingeniously to apply to this kind of experiment.

9.

Admitting first, that the chemical effects which take place in the voltaic pile, and which represent the expense of the magnetic apparatus, are in the direct proportion of the

active surfaces; it appeared to me of the greatest importance to establish the ratio of the surface of a voltaic pair to the weight that a bar of soft iron submitted to the magnetizing power of the current could support. A bar of soft iron $1\frac{1}{2}$ inch in diameter and 29 inches long weighing $14\frac{1}{2}$ pounds, was bent into a horse shoe, such, that the centres of the legs were 7 inches apart. The bar, covered with silk, was inclosed in a spiral of copper wire $1\frac{1}{4}$ ligne thick and 35 feet long. The magnetic force was measured by means of a Roman balance and a weight supported on rollers, so as to slide easily on the arm of the lever. The surface of the soft iron armature was a little convex, so that the extremities of the legs were touched only by a ridge which formed a right angle with the lever. The armature was in connexion with the legs when the lever was placed horizontally. On this latter a scale was graduated, of which the divisions indicated the thirtieth part of the sliding weight, to which was fixed an index. The tenths of these divisions could easily be estimated. I had taken the necessary precautions to avoid as much as possible the errors of observation, proceeding from the disposition of the apparatus. I shall not here enter into the details of this rather complicated apparatus, as I propose giving it in some other part, as it may be useful for experiments of this kind. The electromotors that I used consisted of copper troughs $\frac{3}{4}$ of an inch wide and deep enough to be able to dip into them zinc plates of 4, 16, 36, 64, 100, 144 inches square. The contact of these latter with the copper was prevented by the interposition of pieces of wood. The liquid conductor, of which I had previously prepared a great quantity so as to serve for a series of experiments, was acidulated with 10 per cent of concentrated sulphuric acid of the specific gravity of 1840. The experiments with the same voltaic pair, were made without interruption, but after each one, the precautions were taken to scour carefully the zinc plates, to wash the copper troughs with water, and to renew the liquid so as to restore the same state of action. But further observations have convinced me, that the primitive state is restored more surely by exposing the plates, but especially the negative one, to a current of air until they are perfectly dry. It will then no longer be necessary so frequently to renew the liquid, particularly when we confine ourselves to the observation of the primitive state. I must confess I found upon trial that copper troughs are in reality but ill adapted for electro-magnetic experiments. Concentric cylinders that might be immersed in the liquid would be found much preferable. These cylinders ought to be stiff enough to preserve the proper distance, without being obliged

to have recourse to the interposition of pieces of wood, or any other insulating matter. Much more constant galvanic effects are obtained if the liquid stratum between the two metals be not too narrow: it ought in all cases to exceed half an inch. For the same cause I have made use of voltaic pairs, arranged after the manner of Hare's Calorimotor; but there are also reasons for rejecting them. It is a very different thing to make insulated experiments, and to put the galvanic action in requisition for hours and whole days. It is then that practice requires measures, the necessity of which we had not hitherto the opportunity of foreseeing. It will be necessary entirely to reject the use of copper as the negative metal: we must not spare the expense of using silver, platina, or at least copper strongly plated with silver, gold, or platina. The dissolution of the copper in the sulphuric acid, however weak it may be, and its reduction into the metallic state by the secondary effects of the newly formed hydrogen, make room for some partial galvanic effects by which the principal action is much affected, and to avoid which too much care cannot be taken. Indeed, sometimes the motion of the magnetic apparatus was suddenly lessened or entirely interrupted, and upon nearer examination I found that metallic particles of cementated copper or iron were deposited along the interposed pieces of wood, or on the bottoms of the troughs, and thus formed a partial circuit. On the subject of zinc I shall speak hereafter.

The following is a table of the observations I have made on the magnetic power of the horse-shoe bar described above.

Surface of the Pair.	No. 1. lb.	No. 2. lb.	No. 3. lb.	No. 4. lb.	No. 5. lb.	No. 6. lb.	No. 7. lb.	No. 8. lb.	No. 9. lb.	No. 10 lb.	Force mean lb.	Force calculated lb.
4 in. square	41,46	40,31	47,19	43.18	45,47	48,88	46,52	55,21	55,21	54,07	47,75	47,8
16 do.	126,32	123,43	125,21	125,67	128,47	130,16	124,39	120,55	130 16	130,16	126,45	126
36 do.	156,98	216,54	180,04	189,73	184,34	185,30	205,91	211	157,38	162.77	185	182,3
64 do.	201,72	206,29	195,14	197,33	199,99	198,32	201,25	202,92	197,41	203,33	200,02	216.1
100 do.	266,63	243,21	236,45	235,65	229,93	232,75	232,75	228,79	227,11	227,11	236,05	236,3
144 do.	318,8	221,46	210,17	198,88	210,17	198,88	192,23					
144a* do.	258,56	257,66	254,46	252,12	256,22	253,02					255,34	249

The values given in the last column were calculated by the formula $A = \frac{(233 \cdot 6)\,x}{20 + x}$ the constants of which are found by the method of the least squares. It is true there are some considerable differences between the observations of the same series,

* The first series of experiments, that were made with the pair of 144 inches square, gave so many different values that no use could be made of them. I sought in vain for the cause of these anomalies. After fifteen days the experiments were repeated and gave nearly the same values, which the table proves.

but there was no reason to choose those which agreed best with each other, and attribute the differences to some error in the observation.

10.

I read in an extract from the Memoir of M. l'Abbé dal Negro, which will be found in the "*Annali delle se*, 1813,* *Marzo e Aprile*, 105—120" that this author established the remarkable law, that the magnetizing power is in the direct proportion of the perimeter of the electromoter, and the surface adds scarcely anything to this power. I did not delay making experiments to confirm this law, which appeared to me of great importance for the economical effect of the magnetic apparatus. Two plates of zinc and copper, 36 inches long and 7 inches wide, were bent into a spiral, and separated at a regular distance of a quarter of an inch by small pieces of wood similar to the calorimotor: the whole was placed in water acidulated with 10 per cent. of sulphuric acid. The mean weights that the bar was able to support, by employing this pair of 252 inches square, was deduced from five experiments, and amounted to 297·12 lb.

From the same piece of copper and zinc I cut also two plates, 96 inches long and half an inch wide. The plates were bent in the same manner and separated at the same distance. The mean value of the magnetic force by employing this electromotor of 48 square inches, was also deduced from five observations, and amounted to 133·79 lb. By using a more acidulated liquid the weight could be increased as far as 180·49 lb.

These two experiments could not be ranged beside the others as there are different circumstances connected with them. But the perimeter of the first electro-motive spiral being 86 inches long, and that of the second 193 inches, it does not appear that the law of M. dal Negro was confirmed by these two experiments. There are many emprical formulæ in physical matter which cannot be extended to extreme cases, but at least, they ought to be general enough not to yield to the slightest attempt at increasing their limits. Besides, I have taken the pains to calculate the experiments of M. dal Negro by the formula of M. Ohm. The fourth column of the following table indicates the results according to the formula already found $A = \frac{41 \cdot 55x}{14 \cdot 4 + x}$ in which x represents the surface of the pair.

* Probably 1833. EDIT.

Surface.	Perimeter.	Force observed.	Force calculated.
Square inches.	Inches.	Kilogr.	Kilogr.
6	14	13·85	12·22
12	16	18·20	18·89
18	18	22·80	23·08
24	20	24·60	25·97
30	22	25·80	28·07
36	24	29·60	29·68
42	26	30·30	30·94
48	28	32·80	32·00
54	30	33·00	32·80
60	32	35·60	33·51

The second column of the above table, which contains the perimeter of the plates, represents at the same time the forces by the law of M. dal Negro. This distinguished philosopher did not make these experiments to establish the theory of M. Ohm; but the beautiful concordance of his observations with this theory sufficiently prove that they have been made with much accuracy.

11.

Since 1831, Mr. Faraday has published, from time to time, experiments made to search into the nature of electricity and of its different effects. These experiments ought, for their extent, for the certainty and ingenious sagacity manifested in them, and for the abundance of the results they possess, to be ranged beside the most eminent works which have ever been made in physics. It is by a lucky chance, which I cannot too well appreciate, that these works coincide with the efforts I have made to profit by the mechanical action of magnetism.

Considering a voltaic pair, of copper, silver, or platina and common zinc, placed in acidulated water, a quantity of hydrogen gas will be seen to evolve. The circuit not being closed, this gas will only be evolved at the surface of the zinc; but the circuit being completed, there will also be gas developed from the surface of the copper, or in general of the negative plate. This latter quantity of gas is without comparison less than the former, yet, notwithstanding, it is from this alone that the magnetic power of the connecting wire is derived. The gas abundantly disengaged from the surface of the zinc, contributes nothing at all to this effect. By taking a plate of amalgamated zinc, instead of common zinc, or

some amalgam of zinc, there will only be a development of gas when the circuit is closed; by breaking it this development ceases, the zinc in this combination not being attacked by the acid, or not being capable by itself of decomposing water. It will not be easy to explain this extraordinary fact. In such a pair, all the hydrogen gas, or its equivalent of zinc, serves to produce an electric current, whose magnetic force, calorifying power, and chemical action, are in direct proportion to the gas disengaged or the zinc oxidized; and these different effects may equally serve to measure the quantity of electricity traversing the connecting wire or the apparatus itself. The definite action of electricity, as to the chemical action, in decomposing bodies, is incontestibly proved by the ingenious and numerous experiments of Mr. Faraday. There will be no delay in proving this law as far as concerns the other effects: but the conviction of genius gives right to participate experiments and to pronounce great laws.

Amalgamated zinc is much more positive than common zinc, and its effects are much more decided. Besides, a voltaic pair of this kind has a remarkable constancy, provided there are no secondary effects, proceeding from the precipitation of the negative metal on the positive plate. It may so happen that some portion of the zinc be not well amalgamated; then a direct action of the acid on the zinc will take place; there will be a development of hydrogen gas in this place, the negative metal dissolved by chance in this liquid will be reduced by the gas, and there will be a partial pile, which will affect the principal action. These partial effects will be propagated by degrees over the whole surface, whose positive state will then rapidly decrease. This will not take place if the negative metal be not soluble in the acid.

12.

I have made several experiments on this subject. A zinc plate 7 inches square, weighing 848 grammes was amalgamated, to form a voltaic pair, with a copper plate of the same size. The liquid was sulphuric acid, at the specific gravity 1·105. There was no development of gas at the surface of the zinc: the air bubbles which were formed by degrees rose so slowly that we might very well neglect them, even if we did not believe that they were for the most part atmospheric air contained in the water. After five hours' action, the plate was again weighed, and found to have lost only 112 grammes; during this time the pair had been twice separated from the acid and dried for five or six minutes near a stove.

The following is a table of the deviations of the needle, showing the decrease in the energy of the current.

Time.	deviation.	Time.	deviation.
8h. 12′	61°	10h.	60°
8 22	59	10h. 30′	58
8 30	58	11	57
8 42	57½	The pile	was dried
8 56	56½	11 5	61
9 10	55½	11 30	60
The pile	was dried	12	59½
9 16	62	12 30	58
9 30	61	1	57

The following day the experiments were repeated with the same pair. The decrease in the deviation was not more rapid than before, and it could always be re-established by drying the plates; once it even increased to 65°. At 10h. 50′ in the evening the deviation was still 55°. The action was prolonged during the night, but the next morning we found the plate fallen to pieces. Amalgamated zinc is too fragile to be employed in very thin plates. In order to compare the effects, a plate of common zinc of the same size, was joined in a pair with a plate of copper and immersed in the same acid. The deviation at first was 55½° after 43′ it went down to 12° and by drying the pair we could only re-establish 13°. Having been submitted to the action of the acid for 1¾h. the plate disappeared, and the insoluble parts of it only remained.

I also made experiments on a liquid amalgam of zinc extended in a porcelain basin over a surface of 48 inches square; instead of a plate I used a copper wire 1¼ ligne in diameter, bent into a flat spiral, to allow the gas to escape with more facility. The effects of this combination were very extraordinary. For without touching any, the needle during fifteen hours' action, had only recoiled 11½° from 60°, and was fixed at 49½° After breaking the circuit and exposing the spiral to the air for some time, the deviation was re-established to 59°. This experiment is the more striking, as the multiplier of the galvanometer consisted only of a single coil of copper wire 1¼ ligne thick, for we know that the decrease of the needle is much weaker when a very long and thin wire is employed.

A plate of copper gilded, and one of amalgam of zinc, composed of one atom of zinc and one of mercury, (*Zn. Hg.*) a composition which is sufficiently solid to be employed in

plates, gave also very good effects with regard to the constancy of the deviation and its re-establishment.

To try other compositions, which, according to Ritter, are still more positive than amalgam of zinc, I had plates of lead, pewter, zinc, different alloys of these metals, and different amalgams made of equal sizes. The alloys were compounded atom by atom*, and besides, a plate of each composition was amalgamated on its surface. The direction of the deviation of the galvanometer determined the place in which each alloy was to be put. The liquid in which the plates were immersed was sulphuric acid increased by four times its quantity of water. I must remark that the least change in the surface often affected the place of the metals, of which the electric relation did not differ too much. It is principally in lead and its alloys that this phenomenon is the most strikingly shown. Lead, newly polished, is very positive, with regard to lead, exposed to the air for a few minutes, or dipped in some acid. The following is the result of two series of experiments, that I made with the greatest care.—

— First Series.	— Second Series.
Tin.	Tin.
Lead and tin (*Pl. Sn.*)	Lead.
Lead.	Amalgamated tin.
Amalgamated tin.	Amalgam of tin and lead.
Amalgamated lead.	Lead-pewter.
Amalgam of tin (*Sn. Hg.*)	Amalgamated lead.
Zinc and tin (*Zn. Sn.*)	Amalgam of tin.
Amalgam of lead (*Pl. Hg.*)	Zinc and tin.
Zinc-tin-lead (*Zn. Sn. Pl.*)	Amalgam of lead.
Zinc-lead (*Zn. Pl.*)	Tin and lead amalgamated.
Tin and lead amalgamated.	Zinc, tin, and lead.
Zinc.	Zinc-lead.
Zinc and tin amalgamated.	Zinc.
Zinc-lead amalgamated.	Zinc amalgamated.
Zinc, tin, and lead amalgamated.	Zinc, tin, and lead amalgamated.
Zinc amalgamated.	Zinc-lead amalgamated.
Amalgam of zinc (*Zn. Hg.*)	Amalgam of zinc.
Amalgam of tin and lead (*Zn. Pl. Hg.*)	Amalgam of tin and zinc.
Amalgam of tin and zinc (*Sn. Zn. Hg.*)	Amalgam of tin and zinc-lead.
Amalgam of tin, zinc, and lead (*Sn. Zn. Pl. Hg.*)	Amalgam of zinc-lead.
Amalgam of zinc-lead (*Zn. Pl. Hg.*) +	Tin and zinc amalgamated. +

* In alloys it is customary to unite the metals in regard to weights. I have united them by atoms, bearing in mind the general law of truly chemical compositions.

We see by this, that the alloys and particularly the amalgams are always positive with regard to the simple metals. Most of the amalgams, those of pewter and lead excepted, might be employed in plates. As for the chemical action on these different compositions, it did not take place in amalgamated zinc and the amalgam of zinc, more than in all the alloys and amalgams of pewter and lead; but the liberation of gas was very rapid in all the other compositions of zinc. In the first series the amalgam of tin and lead occupies a very positive place, but the hope of profiting from it is disavowed by the second series. In the employment of amalgamated plates, or the amalgams of zinc, there are several circumstances of which I cannot yet ascertain the cause. During the voltaic action particles of amalgam are often detached in the form of flakes, which float in the liquid and are deposited on the copper or negative plates, so that the latter are by degrees amalgamated; by which the action is considerably weakened or even stopped entirely; for it is very remarkable that copper, silver, and platina, amalgamated at their surfaces, have scarcely any, or at best an extremely feeble power for keeping up an electric current with any other metal. I have often remarked that the first deviation of the needle was very strong, and that it afterwards returned rapidly to its first position of equilibrium without showing any deviation, the voltaic circuit, composed of zinc and amalgamated copper, remaining always closed. It also appeared to me remarkable, that a copper, platina, or iron wire could be much more easily amalgamated under the influence of sulphuric acid, by mercury containing other metals than by absolutely pure mercury. It would be desirable that this object might attract the attention of learned men more competent to make such experiments.

Pure zinc has nearly the same qualities as amalgamated zinc or the amalgam of zinc, being but very slightly attacked by sulphuric acid. It is only subject to chemical action on entering the voltaic combination. I refer for this subject to the important memoir of M. Aug. de la Rive, which may be found in the "Bibliothèque Universelle" of 1830, vol. 43. I have not yet been able to procure pure zinc to repeat these experiments, and to employ it in the magnetic apparatus. In the foundries we might easily obtain pure zinc in large quantities, by distilling it afresh until it is purified of the cadmium and other foreign metals. The price would not be very much increased, but hitherto we have not had sufficient interest in using pure zinc to risk the expense of the repeated distillation. M. Fengler, Pharmacian to Myslowitz, in Upper Silesia, had constructed the necessary apparatus for preparing pure zinc

on a large scale: he could furnish it at 9 crowns a quintal, provided any one would engage to take 3 quintals, but unfortunately his foundry has since been burnt down. His process consists in interrupting the distillation when all the cadmium is driven out, then changing the recipient and again interrupting the process when he thinks the other foreign metals are volatilized or mechanically drawn away. He repeats these experiments as often as he considers it necessary. Zinc thus prepared ought not to be re-cast in iron crucibles.

13.

The rapid decrease of the voltaic effects in ordinary piles, is a great hindrance to the application of electro-magnetism. It may be overcome, at least partly, by an assiduous study of these effects. The motion of my magnetic apparatus has always been very rapid at the commencement, but the velocity quickly decreases and entirely ceases after a lapse of time never exceeding an hour. By employing plates of amalgamated zinc, I have succeeded at three different attempts in making the apparatus work for 20, 22, & 24 hours, without absolutely changing any thing in the pile. The experiments were always interrupted by some accident, and I believe but for that, the extent would have been even much longer. The liberation of gas was very inconsiderable, and only took place at the surface of the negative plate. The velocity was always at first from 120 to 122 revolutions a minute, and afterwards decreased in about half an hour to 62 revolutions; which may be attributed to the commutator not yet having the proper construction. During the remainder of the time the velocity of the apparatus was of a remarkable uniformity, making from 58 to 62 revolutions per minute. I must however confess that it is only these three times that I have experienced so extraordinary an effect. There were always some exterior circumstances, depending on the form of the voltaic apparatus which would contradict the effect. But I should become master of most of these circumstances by constructing a new apparatus whose manipulation would be more commodious and the effect more certain.

14.

We have expressed by $A = \frac{E}{R}$ the magnetic force of each section of a wire traversed by an electric current. We have measured this force either by the deviation of the needle, or the magnetizing power of the connecting wire. Adopting Faraday's law we might equally measure this current by the

liberation of the gas, which at the same time represents the expense attending a voltaic apparatus. D being the quantity liberated, we shall have $D = \frac{E}{R}$ Whence referring to the formula in Art. 8, the economic effect might be expressed by the magnetic power of the whole extent of the connecting wire, divided by the development of the gas. This effect is in nowise changed, either by increasing the surface of the plates, or employing several branches bent in a spiral about different bars of the same dimensions. But the economic effect might be increased at pleasure, by multiplying the spirals, and uniting them so as to form a continued wire. For, employing n spirals or n units of length, the liberation of the gas would be expressed by $D = \frac{E}{nr + r'}$; but we could put in action the magnetizing power of the whole extent of the wire conductor, and we should have for the total force $F = \frac{nE}{nr + r'}$ or $\frac{F}{D} = n$. When the magnetic bars are meant to produce a mechanical motion, the increase of the economic effect will find its limit, since by multiplying the bars we increase the weight of the apparatus and the friction of the pivots, so that this effect will only be expressed by

$$\frac{F}{D} = n - \frac{(nr + r')nf}{E}.$$

The maximum of the economic effect that can be obtained will depend on the value of f or the friction. By differentiating the second member with regard to n we shall have for this maximum $n = \frac{E - fr'}{2fr}$.

The bar used for the experiments in Art. 9 weighed 14½lbs. Being adapted for some moveable apparatus, the friction that it occasioned amounted at most to half a pound. We have found by experiment: $E = 283{\cdot}6$, $r' = 20$, $r = 1$ & $f = \frac{1}{2}$, hence $n = 273{\cdot}6$ that is to say there would be the greatest possible advantage by employing about 273 bars surrounded with spirals of the same size. This number varies with the size of the plates; for a surface m, we have $n = \frac{mE - fr'}{2fmr}$. In short the magnetic power which will serve for useful work, is

$$\frac{nE}{nr + r'} - nf = \frac{(E - fr')^2}{(E + fr')2r}.$$

15.

By employing a voltaic battery, the economic effect will be diminished, unless at the same time we multiply the spirals connected in the same wire. For Mr. Faraday has proved by the experiments related in Articles 990, &c. of the 8th Series of his Researches, that the same quantity of electricity traverses a battery of any number of pairs as traverses a single pair of the same size. The quantity of gas liberated at the surface of each plate of the battery, is the same as that at the surface of a single pair. This appears to be very astonishing at first sight, and seems to contradict the numerous experiments that have been made on the pile; for it is generally known that the quantity of gas liberated in the decomposing apparatus, and at the same time the deviation of the needle increase to a certain point by multiplying the number of the plates. Considering the formula $F = \frac{n' E}{n' r' + r}$ in which n' represents the number of pairs, r' the resistance of each pair, and r that of the connecting wire, or of the body to be decomposed, we must suppose that in the experiments of Mr. Faraday (990) the connecting wire both of the battery and the single pair, was short enough to allow its resistance r to be entirely neglected with regard to $n' r'$. We should not have obtained this striking result if we had employed a long connecting wire, and still less if we had closed the circuits of the pair and the battery by some decomposing apparatus. Mr. Faraday has established a very exact distinction between the quantity and intensity of the electricity put in current. There are different ways by which the first might be measured, but it would be difficult to obtain an exact measure for the intensity, which, nevertheless, is very necessary to complete the theory. Admitting the important law of equivalents in galvanic decompositions, I think we might multiply these equivalents, by the number of pairs necessary for the decomposition. This, perhaps, would be the true measure; for we must consume several atoms of zinc, to decompose a single atom of any other less decomposable substance. What causes the difference between the quantity and the intensity, are analogies in caloric, and judging from a quantity of gas we ought always to know its volume and its density. We must here quote one more observation of Mr. Faraday's, which is in the 7th. Series, Art. 853. He speaks of a current which is, as he says, "*powerful enough to retain a platina wire* $\frac{1}{104}$ *of an inch in thickness; red hot in the air during the whole time.*" ($3\frac{3}{4}$ minutes); and he adds in a note, "*I have not*

stated the length of the wire used, because I find by experiment as would be expected in theory, that it is indifferent. The same quantity of electricity which, passed in a given time, can heat an inch of platina wire of a certain diameter red hot, can also heat a hundred, a thousand, or any length of the same wire to the same degree, provided the cooling circumstances are the same for every part, in both cases, &c." This is all just; but let us add that we must multiply the number of pairs in the same proportion as the length of the wire to obtain a current of the same quantity. In fact, there would be required the liberation of 1000 quantities of gas, proceeding from the same number of pairs, to heat a wire of 1000 inches to the same degree, as would be required for a wire of 1 inch by a single pair. I thought this remark ought not to be suppressed, whilst the practical employment of the voltaic pile requires respect to be paid to economy.

16.

The following is a table of the experiments I have made on the deviation of the needle with regard to the quantity of gas developed at the surface of the negative plate of a voltaic pair of silver and amalgamated zinc. The specific gravity of the sulphuric acid was 1·25.

Deviation of the needle.	Time occupied in liberating a cubic inch of Hydrogen gas.	Deviation of the needle.	Time occupied in liberating a cubic inch of Hydrogen gas.
42° 45′	50″	26° 30′	189″
41 30	57·5	24 52	217
39 30	64·5	23 52	231
34 45	89	23 7	246
32 22	108·5	21 30	290
29	144	20 15	312
27 30	167	20 7	330
27 15	166		

The bubbles of air rise regularly enough to serve as a measure. In another series of experiments I counted the time occupied in developing 10 bubbles of air, of which the following is a table.—

Deviation of the needle.	Time occupied in developing 10 bubbles of Hydrogen gas.	Deviation of the needle.	Time occupied in developing 10 bubbles of Hydrogen gas.
34° 30′	22·″5	18° 22′	80″
32 30	25	15 15	101
31	27·5	14 30	124
22	56	14 20	126
21 37	57	14 10	129
21 22	59	13 20	147
19 37	67	13	160

We must remark that there was a very feeble development of gas even at the surface of the zinc, of which we have taken account. But the quantity of gas measured was in my belief less than that developed, for there was a secondary action manifested by the blackness of the silver plate, and which we have attributed to the metallic reduction of the oxides dissolved in the acid. As it is very difficult to transform the deviation of the needle into forces,* these tables could not serve to confirm the law of Mr. Faraday. We can only perceive by it, that the deviation of the needle follows the same course as the development of gas. I shall repeat the experiments, but reversing the process; viz: ascertain the development of the gas will be taken for the most exact measure of the force of the current, and the value of the degrees on the galvanometer will be deduced from it, either directly, or by some formula of interpolation convenient enough for use. The experiments quoted are not rigorous enough to form the elements of calculation.

17.

Let us return to the magnetic machine. We have succeeded in gaining a reversion in the direction of the current, as instantaneous as exact, by the commutator described in Art 7, whose effect is not at all affected by the velocity of rotation. We have succeeded also in gaining, at least for some time, a tolerably constant voltaic apparatus. In short, means have been found to reduce to a minimum the expense attending it by preventing the direct action of the acid on the zinc, an action which really cannot be made useful, and which as we know infinitely surpasses that which produces the voltaic current. Thus the most important difficulties in the prac-

* Becquerel Traité d'Electricité et du Magnetisme. T. II, p. 20.

tical application of electro-magnetism being overcome, I think it is time to examine more closely the nature of the forces we should wish to make use of, and particularly to ascertain the cause, that limits the velocity which we are right in supposing ought to be infinitely accelerated. This velocity with a pile of 4 pairs, 2 feet square, never exceeded 120—130 revolutions a minute. We must not lightly abandon conclusions founded on the nature of things, and those, which I relate, are drawn solely from the integral $\int_0^a Mds$, expressing the magnetic attraction and considered to be independent of the velocity. It also rests on the legitimate supposition that the electro-magnetic excitation of soft iron, operates instantaneously. If this were not the case, my apparatus would have shown, that magnetism and electricity ought to be attributed to the motion of a tolerably gross matter, or to much more perceptible oscillations than those which propagate sound. In short nobody can deny and that if it be the nature of a force, not to require time to act, and that its different effects were not instantaneously perceptible, it would then be some molecular motion subject to mechanical laws, which took place.

18.

At the end of my first note I said, in the employment of thermo-electric piles for the motion of machines we had to fear the magnetic electrical currents developed by magnetism in motion. The reaction proceeding from it, will be almost entirely destroyed in the hydro-electric pile, the liquid conductors offering too great a resistance to the passage of these currents. These considerations were founded on detached experiments. Making use of a thermo-electric pile, the deviation of the needle was affected by a magnet placed in a spiral forming part of the circuit; it was not so with a voltaic pair of small dimensions. The deviation of an extremely sensible galvanometer was not altered. I was not surprised at that since the conducting power of liquids is much below that of metals. But experimenting on the magnetic force of a bar of soft iron, I sometimes found considerable differences for which I could in no way account. I was curious to know if these differences proceeded from the nature of the iron, or from the weakening of the electric current produced by a voltaic pair of half a foot square of surface. For this reason I placed in the circuit a galvanometer so distant as not to be affected by the direct magnetism of the bar. I was much astonished to see the needle recoil on placing the armature and advance on removing it, for it was the first time I had

known the double quality of the connecting wire, viz: that of conducting the voltaic current and at the same time representing an ordinary wire subject to the action of a magnet in motion. The spiral producing a magnet by the voltaic current is at the same time a magnetic electrical spiral in which a magnet is placed. Hence the solution of the problem of the uniform velocity of the magnetic machine. For being set in motion by the magnetizing power of a voltaic current, it represents simultaneously an apparatus, composed of magnets in motion, and capable of producing a magnetic electrical current, in a direction opposite to that of the voltaic current. This first is closed by the pile itself, which, being composed only of a single pair, does not, as far as we can see, offer too great a resistance to its passage.

I placed a galvanometer in the connecting wire formed by the union of the sixteen spirals of the apparatus. Then closing the circuit and preventing the motion of the machine, I observed the deviation of the needle. It amounted to nearly 60°. As soon as the motion of the apparatus commenced the needle recoiled and continued to do so in proportion as the velocity became more accelerated. The motion becoming uniform at 60 revolutions a minute, the needle was fixed at a deviation of about 47°. The needle always advanced when the motion was stopped or retarded; it recoiled on the contrary when it was mechanically accelerated. It appeared that the deviation of the needle at 47° corresponds to the state of equilibrium, for the motion ceasing by itself the needle did not quit this position. Also in several experiments, whether the first deviation of the needle exceeded 60° or whether it was less, it always became fixed at about 47°. The voltaic current being weakened by the interposition of different branches, until the first deviation of the needle amounted to no more than 47° the magnetism was not strong enough to produce motion in the apparatus. Repeated experiments will be necessary to explore these interesting phenomena.

19.

I thought it would be profitable to open to the magnetic electrical current two passages or separate branches, one of which would be the pile and the other a second metallic connecting wire, so long and so thin as not to affect too much the quantity of electricity passing by the principal connecting wire, (Art 8 No. 6). It was natural to suppose that the counter current would rather follow the metallic wire than the liquid of the pile. It was not so. During the motion of the apparatus the galvanometer being fixed at 47° and the second circuit

suddenly established, the needle was not much affected by it; it certainly advanced but only 1·5°; also the velocity of the apparatus was not sensibly altered. On reducing the length of the second wire nearly the same thing took place. The passage of the counter-current over the metallic wire, was proved at least in part by the interposition of a second galvanometer. During the accelerated motion the needle of the latter advanced in proportion as that of the former recoiled. We ought to expect it, since the counter-current in the secondary branch has the same direction as the voltaic current. This is quite conformable to the remark M. Nobili has added to the end of his first memoir on the theory of electro-dynamic induction (*Antologia di Firenze*, 1832, *No.* 42.) The ends of the connecting wire surrounding the bars, ought to be considered as the poles of an electro-motive apparatus. In addition to this the magnetizing power of this counter-current has been proved by making it traverse a spiral bent about a bar of soft iron.

20.

In short every thing leads us to believe, that the greater part of the counter-current might be made useful by employing two apparatus of the same kind, whose connecting wires, bent in spirals about bars in each system, terminated in the same pile. The counter-current engendered by the motion of one apparatus, would serve to reinforce the magnetism of the other, and vice versâ; the counter-currents would balance one another to annihilate their effects. An experiment might be made on a small scale with the bar described, the legs of which were surrounded by separate spirals. Fig. 114, shows the disposition of the experiment. The two spirals were connected by the pointed wire *c b* immersed in the small cups *c b*, filled with mercury. They thus formed a single connecting wire, of which the other ends *a d*, were combined with a pile C Z. I grasped in my hands, moistened with acidulated water, the connecting wire at the place *e*, *f*, and I caused the circuit to be broken at *g* or *h*. I felt a violent shock. This is otherwise the same as the beautiful experiment of Mr. Jenkins, related by Mr. Faraday.* Placing the multiplying wire of a galvanometer *m* in the circuit, the needle was deviated to 48° by the voltaic current. Then placing the armature near, it recoiled from 48° to 40°. The deviation on taking away the armature could not be observed, the latter being too strong. The spirals were now connected with the pile in two

* The experiment is originally that of Professor Henry. See his paper in this volume. Edit.

separate branches by means of the wires *a b* and *d e*. The wire *c b* was taken away. I expected on breaking the circuit, to perceive that the magnetic-electrical current excited in the spiral *a c*, would be conducted entirely by the spiral *b d* and vice versâ. But I was wrong. The shock was not much less. The needle did not recoil less. I was struck with this experiment, but after all I considered this magnetic-electrical disposition as an unclosed voltaic pile, consisting of two elements combined so as to form only a single pair as represented in fig. 115. The currents, whose directions are opposed with regard to the wires *a b*, *c d* unite, in crossing some connecting wire placed in contact with the points *e f*. If the galvanic excitation be not in perfect equilibrio, being stronger on one side than the other, there will be a deviation of the needle proportional to the difference of the currents which traverse the wires *a b*, *c d*. This is conformable to the experiments related by Mr. Faraday, in the beginning of the 8th *series*, on the subject of decompositions made with a single voltaic pair. In fact what is called tension, is the effect of forces equal and contrary in direction. In mechanics such forces destroy one another, their sum being zero, but in natural philosophy it is another thing.

As to the direction of the magnetic electrical current which causes the shock, it is the same as that of the voltaic current. That was proved by a galvanometer, the multiplying wire of which terminated in the points *e*,*f*. There was a deviation on a part of the voltaic current, traversing the secondary branch *e*,*f*. On causing the armature to approach, the needle of this galvanometer advanced at the same time that the needle *m* recoiled. The contrary effect might be observed on removing the armature by a blow from a hammer.

21.

Here are still some more experiments relative to this subject. The extremities of the bar were surrounded by a copper-plate fig. 116, in the circuit of which was placed a galvanometer. On causing the armature to approach, the needle was not affected; but after having bent the ends of the multiplying wire about the points *e*, *f*, and the circuit being thus closed, there was a considerable deviation.

An analogous result will be found in the following experiment. Immersing in the cups *a*, *b* or *c*, *d* of the bar, fig. 114, two plates of copper, which were firmly grasped in the hands, there was no shock, when the circuit was broken by separating the wires *a b* or *c d*; for the human body formed part of a circuit in which equal excitations took place on two opposite sides.

The plates being immersed in the cups *c* and *b*, there was a violent shock at the moment of the disjunction.

I formed a thermo-electrical circuit of bismuth and antimony, in which was placed a galvanometer; after having heated the two solderings to the same degree there was no deviation of the needle; but the multiplying wire having been placed so as to form an intermediate branch, and the solderings being at opposite sides, there was a considerable deviation. That would not have happened if the circuit of bismuth and antimony had been in its normal state, for it would have then conducted the greatest part of the thermo-electrical current, since the multiplying wire was so long and thin as only to intercept an extremely feeble part.

It seems to me that there are circumstances which cause metals to lose their conducting power, and these same circumstances on the contrary increase that of liquids. Is this perhaps the state of bodies that Mr. Faraday calls electro-tonic?

22.

In the supplement to No. 105 of the Institute for May 13, 1835, will be found a note on a memoir by Mr. Faraday, which we expect to be published. The experiment described at the end of this note seemed to me so striking and important to the researches which are the object of the present memoir, that I did not delay to repeating it. Two copper wires, 400 feet long and $\frac{8}{4}$ of a ligne in diameter, carefully covered with silk ribbon, were bent together in a spiral about a cylinder in hollow wood of $1\frac{1}{3}$ inch in diameter. The ends of these two wires were united. The effect of this combination exceeded my most sanguine expectations; for employing a voltaic pair of silver and zinc, having only half an inch square of surface, I obtained at the moment of disjunction a bright spark, and so powerful a shock that I could scarcely support it. The same effect took place when the pair was reduced to a platina and zinc wire. After having placed a soft iron cylinder in the hollow of the wooden one, the action was still more considerable. These effects were not much increased by enlarging the surface of the pair. A wire conductor 400 feet long being employed alone, the spark and shock were much more feeble; but after uniting the two ends of the second wire of 400 feet in the circuit there was neither shock nor spark. This is perfectly conformable to the experiment of Mr. Faraday.

For this reason I made the following experiment: in the wooden cylinder I placed a cylinder of soft iron $1\frac{1}{3}$ inch in diameter, forming the armature of the soft iron bar. Let us

call the corresponding extremities of the bar and armature A, *a*, B, *b*. The two spiral wires 400 feet long bent about the armature were united in one of 800 feet, the ends of which were conducted by a multiplier to the poles of a voltaic pair of about $\frac{1}{4}$ of a foot square. The spiral surrounding the bar terminated in a pile of a foot square by means of a vibrating commutator. The deviation of the needle was 16°. The current which magnetized the horse-shoe iron bar, being directed so as to produce in A the same magnetism as in *a*, (A_n a_n, B_s b_s) the needle advanced to 30°, and on reversing the current so as to produce contrary magnetisms (A_s a_n, B_n b_s) the needle recoiled from 16° to 10° returning after a few oscillations to its former position of 16°. Employing a single wire 400 feet, the other wire not forming the circuit, the deviation of the needle was 21°. By the arrangement (A_n a_n, B_s b_s) the needle advanced as far as 33½°, it recoiled on the contrary to 13° when the magnetism of the bar and armature attracted each other (A_s a_n, B_n b_s) After uniting the second wire of 400 feet in the circuit, the deviation of the needle being the same as formerly, viz: 21°, the latter by the above-mentioned arrangements advanced and recoiled to 30° and 14° respectively. We see that in this case the needle is rather less affected than in the case of the disjunction of the second wire; but I had expected, as a necessary consequence, that the needle would not be at all affected, having neither received a shock or spark in the analogous experiment. I confess I am at present unable to explain the striking difference, which subsists between the current of reaction and the magnetic electrical current.

23.

With regard to the magnetic apparatus, it will be very important to weaken the effect of the counter-current, without at the same time weakening the magnetism of the bars. Well: it is the alternate combination of the pairs of the voltaic pile that enables us to increase at pleasure the velocity of rotation. We know that the magnetic power of the current is not sensibly increased by augmenting the number of pairs, but the counter-current is considerably weakened by it, being obliged to pass through a great number of liquid strata. In fact, by using 12 voltaic pairs, each six inches square instead of 4 copper troughs as I had done hitherto, the velocity of rotation amounted at least to 250—300 revolutions a minute, which number I could only appreciate, not being able to count them. The acid I used was extremely weak, and had served for experiments for a long time. The development of gas was

neither appreciable by sight nor smell. Having immersed two large copper wires in the cups *p* and *o*, and grasped them in my hands, moistened with salt water, I received during the motion of the apparatus violent shocks, and felt an extreme pricking in the upper part of my body. The mechanical effect of the apparatus, corresponding to the velocity of 250 to 300 revolutions a minute, was estimated at half the force of a man. I shall apply it hereafter to an exact dynamometric apparatus.

I have not been able to make any further experiments on this subject, and am even obliged to interrupt the course for some time: but from what precedes I think I may assert that the superiority of this new mover is placed beyond a doubt, as regards the absence of all danger, the simplicity of application, and the expense attending it.

LXX. *Facts and observations for the purpose of illustrating a theory intended to connect the operations of nature upon general principles.* By P. Cooper, Esq.*

1. The theory which we propose to illustrate in this paper, will ultimately derive the strongest evidence of its truth from the generalization of phenomena which have hitherto formed several distinct classes; and by connecting these phenomena with the great operations of nature to which they belong.† It is intended, however, that in the progress of its development its consistency shall be supported by experiment, or by showing its application to known facts; and the reader will greatly assist me in the undertaking by pointing out any failures in the fulfilment of this intention, by the opportunities it will present for further explanation.

Those who cultivate science, with a view to discover in the works of nature phenomena beyond the reach of human comprehension, will be disappointed in this theory; the wonders which are calculated to make the vulgar stare and the rational hesitate form no part of the system before us. It must have been observed, that as machinery of human invention is improved by the alterations which are made from time to time to adapt it more completely to its object, it becomes more simple in the same degree that it approaches to perfection; and, corresponding with such observations, it will be found that the machinery of nature being absolutely perfect, is in the highest degree simple. So far then, from its being de-

* Communicated by the Author.

† See abstract of a series of papers entitled "Outlines of a Theory intended to connect the operations of nature upon general principles.

rogatory to the Great Mind which planned the universe, that the most simple means are universally adopted, it is a proof of the highest wisdom.

Having made these preliminary remarks, we shall proceed to the object we have in view.

2. There are few principles in philosophy which appear to be more clearly established than the doctrine of definite proportions; and, consequently, the division of matter into atoms, varying in bulk, or specific gravity, or some other property attached to them, so as to give them different weights when compared with each other.

In the abstract of this theory, lately published, I have assumed that bodies are formed of matter consisting of globular atoms of different sizes; but, in the work from which the abstract is taken, it is stated that the same effect would be produced by uniformity of size and difference of attractive force; and it may be found necessary, in order to account for the various phenomena exhibited in chemical combinations, to admit both these distinctions. The theory was formed during the infancy of the doctrine which now forms an important part of our chemical knowledge, and I have not hitherto disturbed its original simplicity by any attempts to bring it into unison with discoveries which, probably, are not yet fully developed.

3. The next assumption is, that light, also, consists of globular atoms, uniform in size and in every other natural quality, having an attraction for the atoms of matter, and a repulsion between themselves.* This, with the exception of a definite arrangement, is not by any means new; it is almost impossible that the known properties and universal presence of light and heat should not have suggested something of the kind to those who were acquainted with the various operations in which the materiality of this element is so clearly demonstrated. Lavoisier, in his Elements of Chemistry, page 70, says, " It is by no means difficult to perceive that this elasticity depends upon that of caloric, which seems to be the most eminently elastic body in nature. Nothing is more readily conceivable than that one body should become elastic by entering into combination with another body possessed of that quality." And near the close of the same chapter, he says, " When we are once permitted to suppose this repelling force" (namely, the mutual repulsion of the atoms of caloric) " the theory of the formation of gases, or aëriform fluids,

* For a more particular statement of this part of the theory, see Abstract, paragraphs 1 and 2; or Annals, &c. Vol. I. page 231.

becomes perfectly simple; though we must, at the same time, allow, that it is extremely difficult to form an accurate conception how this repulsive force acts upon very minute particles placed at a great distance from each other."

The difficulty which Lavoisier here speaks of arose from the want of any connexion, in his view of the subject, between the atoms of matter and the atoms of caloric, or light; for without such a connexion, which is provided in the present theory by the attraction of matter for light, the mutual repulsion of the atoms of the latter would be unavailing as it regards the elasticity of the former. With this provision, nothing can be more simple; the interposed light attracts the contiguous atoms of matter, and draws them together, independently of any attraction between themselves; while the mutual repulsion of the atoms of the same light causes them to recede from each other; it is therefore at once the bond of union and the motive for separation.

4. I need not say that in this enquiry we have the same liberty to magnify in our imagination the minute objects which are the subjects of investigation, provided we preserve their relative proportions, as we have to reduce the objects which form the solar system; in order, in both cases, to bring them within the limits of our comprehension. The Great Being who created the whole, views an atom and a world with equal ease; and would as readily discover in the former, as in the latter, the slightest deviation from the laws by which their motions are regulated.

If we duly contemplate these minute wonders of creation, recollecting that the smallest animalcula which the microscope renders visible to us, has many members; and that these members must have organs to supply them with the fluids required to support and repair the complicated machinery which is necessary to their various objects; that these fluids and the vessels in which they circulate are formed of an arranged collection of these atoms with the properties we have described, and that *every atom* has its office, the duties of which are performed with the most perfect regularity; it will raise in our minds a greater degree of astonishment than the contemplation of the myriads of worlds which surround us. We can much more easily comprehend the collection of vast quantities of matter into masses, than we can follow the division of matter into those minute receptacles for it, which the eye of reason only can penetrate.

5. As the combination of matter with light is the very foundation of our theory, and the great principle upon which it wholly depends, we may be allowed to be somewhat parti-

cular in our description of it, and of the arrangements which must arise from the influence of the laws by which it is regulated.

If we introduce a simple atom of matter to a quantity of light, the light, from its great attraction for matter, will rush towards it in every direction; but previously to its coming in contact with the atom of matter, it will be met by the mutually repulsive force of the light which is arriving in opposite directions, and, instead of closing upon the atom, it will form a hollow sphere at some distance from it. Every atom of light which forms the stratum immediately surrounding the atom of matter, will be equally distant from the central atom, because the attraction of this atom is the same in all directions; and equally distant from each other; the repulsive force of the atoms of light being uniform, and this force being counteracted by a central attraction, which is also uniform. The stratum of light succeeding that which immediately surrounds the atom of matter, will also be subject to equal forces; it will be attracted by the atom of matter, and it will be repelled by the first stratum of light; but both these forces being equal at equal distances from the centre, it will form a second hollow sphere, at some distance from the first; the next, and every succeeding stratum, will also be subject to equal forces, both of attraction and repulsion, at equal distances from the centre; and the whole will form concentric spheres, at distances from each other depending upon the relative power of the opposing forces. (3; and note with reference.)

6. It is evident that the spheres of light at different distances from the centre, will be held in their position by very different forces; not only because the force of attraction of the central atom declines as the square of the distance, but also, because the repulsive force of the inferior strata of light acts in opposition to this attraction; whereas, the repulsive force of the superior strata concurs with the central force. It is equally evident that at a certain distance from the atom of matter these two forces, namely, the attraction of the central atom and the repulsion of the light which surrounds it, will become perfectly equal, and the atom will cease to exercise any force upon the light beyond it. The light which is thus attached to an atom of matter may very properly be called its atmosphere. It is necessary to observe, and it is a very material distinction, that the state of neutrality we have described does not arise from any cessation in the action of the two forces, but from their exactly balancing or counteracting each other. Hence it is, that as both the forces are regulated by

the same law of distance, although the light which surrounds every atom of matter attracts the atoms of distant matter and repels the atoms of distant light, these two forces being equal at all distances, light or heat, when added to bodies appears to be imponderable.*

7. We have observed that the concentric spheres of light which form the atmospheres of the atoms of matter are held in their position by different forces at different distances from the centre; the force declining as the light recedes from the atom of matter. In consequence of this difference of force, the light which forms the different strata will be in a different state of intensity, or compression; by which we mean, that the atoms of light will be at different distances from each other under the influence of different forces; for the repulsive force of these atoms will be opposed by the force which draws them towards the centre, and, at the same time, nearer to each other. In each stratum of light, whatever may be its intensity, or state of compression, the distance of the atoms from each other will be exactly equal; the forces to which it is subjected being in every respect the same.

8. Having described a single atom of matter, with its atmosphere of light, we will now suppose an unlimited number of these atoms to be brought together, in order that we may observe their action upon each other. If we direct our attention to one of these atoms, we shall perceive that its atmosphere will be repelled by the atmosphere of every atom which surrounds it; but while these atmospheres are all equal, and the distance to which they extend unlimited, the forces thus brought into action can make no impression, because they are everywhere counteracted by equal forces: whatever force is exercised on one side by one atom, is resisted by an equal force on the opposite side by another atom; and this being the case at all points and in every direction, the atmospheres of the atoms preserve their uniform state.

9. If, instead of supposing the atom we have in our contemplation to be surrounded with other similar atoms to an unlimited extent in every direction, we suppose that on one side of this atom, though, perhaps, at a considerable distance, these atoms terminate, and others are introduced, the atmospheres of which present greater or lesser forces, the effect will be very different; for the light which forms the atmosphere of the atom will then flow towards this side in the lat-

* The anomaly which has hitherto existed, between the imponderable quality of caloric and its known attraction for matter, is thus removed.

ter case, and from it in the former; not because there is any direct attraction or repulsion between the distant body of atoms and the atom we have in view, but because every intermediate atom will be brought into the same state; it being impossible to present two different forces to each other without producing this effect to an extent which will exhaust the inductive force, by the resistance which, though with little effect severally, every atom opposes to this derangement of its atmosphere. The body which in this case presents the greater force, is said to be positive, and the body which presents the lesser force, negative.

10. The means by which this extensive derangement is effected may be easily understood. Let us suppose that the atoms A, B, C, D, E, F, &c. in Scheme 1, present

Scheme 1.

POSITIVE.							NEGATIVE.					
A	B	C	D	E	F&c.	|	G	H	I	K	L	M&c
—o+	—o+	—o+	—o+	—o+	—o+	|	—o+	—o+	—o+	—o+	—o+	—o+

greater forces than the atoms G, H, I, K, L, M, &c. or that the former are positive when compared with the latter, which, consequently, must be negative. Let C be the atom we have in view, forming part of the series A, B, C, &c. the atmospheres of which previously to the introduction of the negative body of atoms, G, H, I, &c. were uniform. Now, the moment G and F were brought within the inductive influence of each other, we shall observe the atmosphere of C will flow towards the negative body, and assume the form indicated by scheme 1, by the positive and negative signs; this will happen, not in consequence of any attraction between the atmosphere of C, and the distant body G, H, I, &c., but because the intermediate atoms, commencing with the atom nearest to G, are brought into the same state; the atmosphere of F repelled on one side by the atmosphere of E, and on the other by the atmosphere of G, finds least resistance in G and flows towards it, thus, by rendering the side of F towards E negative, preparing the way for the atmosphere of D to flow in the same direction; leaving it negative towards C, the atom we have been attending to; which, of course, will assume a similar arrangement.

The positive body will exercise a similar, though opposite, action, upon the negative body; the force of F being greater than the force of H, the light which forms the atmosphere of the intermediate atom G will flow towards the latter, rendering it positive towards H, and negative towards F: this will renew the action between F and G, by increasing their

relative positive, and negative states, and there will be a succession of these actions and reactions, which will extend throughout both series entil an equilibrium be established. (See Electricity, 104.)*

The propagation of the deranging force, in the manner we have here described, has usually been called induction; but we shall also occasionally term it the extension of derangement.

11. The deranging force is of so much importance to our theory, that we may be allowed to be somewhat particular in its description. Hitherto we have spoken of the formation of one kind of atoms only; but we have assumed that they are of different sizes, or that they differ in attractive force (2), so that, although the arrangement of their atmospheres is made upon the same principle, the forces which they are prepared to bring into action are different, when compared with each other, both at the surface and at different depths below it, as we approach the atom of matter. There are so many contending forces to be taken into the account, that it is extremely difficult to determine whether the larger or the smaller atom will present the positive surface; it is not impossible, and there are circumstances which render it probable, that to a certain depth from the surface, the atmosphere of an atom may be positive or negative, when compared with an atom of a different size or power, and below this point, or nearer to the central atom, it may be neutral, or even in an opposite state to its surface; at any rate such an arrangement is not inconsistent with the forces which are brought into action. But, at present, it is only necessary to observe that the atoms, thus differently constituted, may reasonably be expected to present to each other different forces, when brought into contact, as at F, G. Let us, then, suppose that the force of the positive atom, F, is 100, and the force of the negative atom, G, 80; or that the intensity of the light upon the two surfaces is in such proportions, that at the points in contact, F presents the repulsive force of 100 atoms, and G the repulsive force of 80. Now, every atom of light repels every other atom of light; the force of 100, therefore, in F is resisted by a force of only 80 in G, and as H presents the same force, the atmosphere of G is repelled towards it, until its increased compression, on the side towards H, produces a resistance corresponding with the force which is opposed to it.

* We shall frequently have occasion to refer to the treatises published by the Society for the Diffusion of Useful Knowledge, and to avoid repetition, we shall merely give the title of the treatise, and the number of the paragraph, the former, generally, as it is abreviated in the index.

12. If we recollect that the light which forms the atmospheres of the atoms of matter has perfect freedom of motion, and that its uniformity can be preserved only while the forces are equal, we must perceive that the derangement of these atmospheres must be a necessary consequence of bringing a force to act upon one side of it which is not counteracted by an equal force on the opposite side. But there is another effect of this unequal action, and consequent derangement, which is highly important; the atom of matter preserves its central position within its atmosphere only in consequence of its being surrounded by equal forces; when therefore the light is repelled from one side of this atom, and accumulated on its opposite side, the atom moves from its central position, to a position corresponding with the relative strength of the opposing forces; so that the atom and its atmosphere are constantly in equilibrium. This is an essential part of the arrangement, and secures its stability, for if it were possible to fix the atom in its central position, it would be found that the resistance to the derangement of its atmosphere arising from it, would prevent those arrangements which are absolutely necessary in the various operations of nature, and which are now made with so much facility, that in some of these operations they are extended to distances of which we can form little conception.*

13. We have hitherto supposed that the light which forms the atmospheres of the different atoms has such freedom of motion, that it may readily take whatever position is required by the forces in action; but, in electrics, in consequence of a difference of capacity in their constituent atoms, and other circumstances which will hereafter appear, this is not the case; and the phenomena of electricity arise almost entirely from circumstances connected with it; for if the atoms A, B, C, &c., and G, H, I, &c., could in every case return to a neutral state upon the removal of the deranging force, as they do when both are conductors, these phenomena might have been for ever unknown to us.

14. Instead of supposing A, B, C, &c., and G, H, I, &c., to be separate atoms, let us consider them to be in a state of cohesion, and forming sections of the glass cylinder of an electrical machine and its rubber, the atom A being supposed to form part of the internal surface of the cylinder, and the atom F part of its external surface, the intermediate atoms forming the section perpendicular to these surfaces; the atom G, part of the external surface of the rubber, which

* Sound, which is propagated upon the same principle, has been heard at a distance of 200 miles.

is continued perpendicularly to this surface, by the atoms H, I, K, &c., to an indefinite extent, terminating with the ground, a reservoir of light, which, as compared with the quantity required in our experiments, is of almost infinite capacity.

15. When the electric, A, B, C, &c., is brought into close contact with the rubber, G, H. I, &c., the atoms of both assume the arrangement described in the scheme; for it must be observed, the positive and negative state of the opposite surfaces of the atoms, indicated by the symbols attached to them, is not intended to show their natural state, but the state to which they are brought by their action upon each other.* The pressure of the cylinder upon the rubber, *by bringing the forces into closer contact*, increases the derangement of the surfaces, and consequently the quantity of light, plus on one surface, and minus on the other, in the direct ratio of the pressure, to a certain point, after which it is probable that this relation diminishes.† The friction is probably of no other use than to transfer the pressure from one part of the cylinder to another, and at the same time break the contact between the cylinder and the rubber, with the necessary rapidity to prevent the return of the cylinder to its natural state, which

* This forms one of the great distinctions between the present theory and every other with which I am acquainted; there is no fixed polarity in the atoms of matter, but the polar forces are induced, by the action and re-action of the positive and negative surfaces upon each other, wherever they happen to be in contact. Hence atoms are not confined to an union in single proportionals, as they probably would be when in a solid state, if each of them were limited to three polar axes at right angles to each other, or to any other proportions which would necessarily arise from these axes being fixed in other positions (See Dr. Prout's Bridgwater Treatise, page 42.); but as the poles are induced wherever atoms with different forces come into contact, they are unlimited in number, as well as in position. If we duly consider the numerous forms of crystallization, which the same atom is capable of assuming in the various combinations into which it is known to enter, it appears to me, we cannot fail to discover the incompatability of a fixed polarity with the phenomena which these changes exhibit.

In the case before us, the polarity assumed by the different atoms, though the same in principle, is altogether independent of the polarity by which these atoms are kept in a state of cohesion; and it furnishes an instance of the manner in which polarity is induced, wherever and under whatever circumstances two bodies, having different electrical forces, are brought into contact.

† See an interesting paper by Becquerel, on the development of electricity by pressure, from which these proportions are taken. Annales de Chemie, Tom. XXII, p. 5.

it would otherwise do upon quitting the rubber, by transferring the fluid upon its surface to the ground, through its medium as a conductor. Becquerel found by experiment, that in the development of electricity by pressure, the effect was greatly influenced by the degree of rapidity with which the bodies were separated, and that when this rapidity was much abated, particularly when the subjects of experiment were good conductors, the electricity became imperceptible. He also found that while the pressure was continued upon the two bodies in contact, neither of them gave the least sign of electricity: a proof that the electricity developed, arises from forces which are permanently in action during the continuance of the pressure.

The method of Becquerel and that by the common electrical machine, differ from each other only in the means by which the pressure is removed and the electrical fluid collected; in both these respects the machine has decidedly the advantage, both objects being effected by a simple motion, which at the same time transfers the pressure to an uncharged part of the cylinder; this however does not in the slightest degree lessen the value of Becquerel's experiments, which were made for a different purpose.

16. Having then explained the means by which electricity is developed, we shall next proceed to trace the principles upon which it is transferred, and more permanently retained. While one surface of the glass cylinder is in contact with the rubber, it will be observed that its opposite or interior surface is in contact with the air, and this being a non-conductor, there is a resistance to its derangement which by re-action prevents the full derangement of the surface in contact with the rubber; it being impossible to give a full charge to one surface of glass positively, unless the opposite surface be charged in a corresponding degree negatively.

If the atoms A, B, C, &c., be a section perpendicular to the surfaces of a Leyden jar, A being the terminating atom of the external surface, and F the terminating atom of the internal surface, it will be impossible to charge F positively, unless we at the same time charge A negatively. That is, we cannot transfer the light which has been developed upon the surface of a glass cylinder, by the action of a rubber, to the interior surface F of the jar, unless we provide the means of escape for an equal quantity of light from its exterior surface A. These objects it is well known are usually effected by transferring the fluid collected from the cylinder, to the interior surface of the jar by means of a conductor, both surfaces having been previously covered with tin foil, while the

external surface of the jar is in communication with the ground, either directly or through the medium of another conductor.

17. Let the series of atoms represented in scheme 2 be a section of one of these coated jars; the glass being represented by the atoms A, B, C, &c., and the coatings by the atoms W, X, &c., and Y, Z, &c.

Scheme 2.

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 W     X          |  A     B     C          |  Y     Z
+o— +o— &c. | +o— +o— +o— &c. | +o— +o— &c.
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When the jar is charged as represented in the figure, the atoms W, X, &c., being the positive coating, and the atoms Y, Z, &c., the negative coating, every atom in the series will be arranged so as to present to each other alternate positive and negative surfaces. To bring them into this state, the atom W upon being charged with additional light upon its exterior surface, will resign the same quantity from its interior surface to X, and X to any other atoms in the coating in succession, until it arrives at the glass, when the charge will be communicated to A, from A to B, from B to C, &c. to the opposite surface of the glass; from which it will be transferred to the negative coating Y, X, &c. in the same order; a quantity of light equal to that which was added to W, being transmitted to the ground from Z. But it must be observed, that the charging and discharging of the different atoms are simultaneous actions, it being impossible to charge W positively, without *at the same time* charging Z negatively, or without taking from Z the same quantity of light which is added to W.

It is easy to perceive, that with this arrangement, when the two coatings are connected by bringing W and Z into communication by means of a conductor, the light will flow from the positive to the negative surface; and as the internal atoms will compensate each other by a similar transfer, the whole will return to a neutral state.

It must be again observed, that an atom of light cannot leave W, unless it be at the same instant added to Z. How then, it may be asked, is this provided for, seeing that the light has to pass through a conductor, which is sometimes of great length? Why, simply by throwing the light which by surrounding every atom of matter, forms a constituent part of the conductor, into the form of a current; by which means the moment the light enters the end of the conductor at W, a corresponding quantity is expelled from the other end in contact with Z.

When we convey gas through a tube to a lamp, perhaps some miles distant, we do not pass it through the similar gas with which the tube is already occupied; but we press this gas forward, by supplying from the gasometer at one end of the tube the same quantity that is emitted to supply the lamp from the other; and the lamp may be burning for several hours before the gas which issues from the gasometer, at the moment it is lighted, reaches it, to supply what it consumes.

Hence we may account for those appearances which have been brought forward to prove the immense velocity of light, and other equally improbable circumstances connected with it; the time which elapses between the light leaving one point and appearing at another, is required (in consequence of its elasticity) to bring it into a state of compression sufficient to give it force to overcome the resistance opposed to its motion, and it must of course be regulated by the supply, and the distance of these points from each other; but if light were perfectly inelastic, it would appear at one point at the same instant it is put in motion at the other, however distant these points might be from each other. The gas tubes again furnish a parallel case.

18. If the coatings of the jar are so constructed that they may be removed from the glass without communicating with each other, they will return to their neutral state as soon as their connexion with the electric is broken; but the electric, unaffected by their removal, will retain its charge, and upon replacing the coatings, they will instantly resume their previous state of derangement. (*See Electricity*, 121.)

19. The action which the electric is here found to exercise upon the coating, is extended to all bodies indiscriminately, until the deranging force is exhausted. If, for instance, several layers of tin foil be added to the coating, all of them will be brought into the same state, by their inductive influence upon each other; and their atoms throughout will present alternately positive and negative surfaces, which upon their removal from the electric, will again return to their natural state; but if instead of additional layers of the metallic coating, a plate of glass be brought into contact with the excited electric, or its coating, although the deranging force in action upon it will be the same, the want of conducting power in the glass will prevent the transfer of light from one surface to the other, which is required to complete the arrangement, and the glass, if it be perfectly insulated, will remain unexcited. If in this state of the experiment, a communication be made between the opposite surfaces of the second plate of glass, (which for this purpose ought to be coated), without interfer-

ing with the charge of the first, which is supposed to be completely insulated, the second plate will instantly become charged, and its atoms will be arranged in precisely the same order as the first, or as the additional coatings already mentioned; but upon its removal from the electric, this charge, unlike that of the tin foil, will be carried with it; and the second plate will be found, with a slight diminution of force, as effective as the first in extending the derangement to other bodies.

20. It will be observed in this and other experiments, that the deranging force is perfectly independent of the transfer of light, although this transfer is necessary to carry the arrangement into effect; the force, consequently, under circumstances in which this transfer is prevented, is suspended; but like a spring opposed by a resistance superior to its own force, constantly exerting itself to overcome the obstacles opposed to its action. We may infer from this, and it is an important distinction, that the transfer of the electrical fluid or light, is not the cause, but the effect of the derangement which is observed to attend it: if this transfer could be made independently of the deranging force, it would produce only an increase of temperature.

In the experiment preceding these observations (19), it will be found upon examining the original electric, that its force is unimpaired by its action upon the second; which not only corroborates our inferences, but also points to a method of increasing the deranged surfaces to an almost unlimited extent, simply by bringing these surfaces into juxtaposition. This mode of exciting electricity, is said to communicate the charge by induction, to distinguish it from other methods of producing the same effect; but upon investigation it will be found that the principle we have here discribed, however the circumstances may be varied, extends to the whole of these methods. (15).

21. When the fluid is collected upon the glass cylinder of an electrical machine by the usual mode of excitement, the want of a corresponding derangement upon its interior surface, not only prevents its acquiring a full charge, but the resistance arising from it, disposes it to part with that which it has acquired to other bodies around it, and a charge can be retained by it for a short time only; but when it is transferred to a Leyden jar, this resistance is removed by a corresponding derangement of both surfaces; when the quantity of light upon the surfaces of the different atoms being, though differently arranged, precisely the same as in their natural state, and the atoms of matter brought into a position of equilibrium with

their respective atmospheres (12), this disposition is considerably abated, and the charge will be much more perfectly retained.

22. We cannot succeed in adding light to the surface F, of the atoms A, B, C, &c., scheme 1, which we have supposed to be the section of a glass cylinder, unless a corresponding arrangement be made in the atoms E, D, C, B, A; but the glass being a non-conductor and in contact with the air, which is also a non-conductor, the resistance to this arrangement will rapidly exhaust the inductive force, and the surface A, will be deranged negatively in a much less degree than the surface F is deranged positively, unless the means of escape be provided for the superfluous light from A. But the atoms, G, H, I, &c., which we have supposed to be a section of the rubber, and consequently, when connected with the ground, an endless conductor, can dispose of any superfluous light, or obtain a supply at any point at which it may be required; and although there will still be a resistance to derangement, the surface M, will approach much nearer to a correspondence with the surface G, than the surface A to the surface F, supposing these two points to be equally distant from each other. In consequence of this distinction, electrical derangement will be extended by induction to much greater distances in conducting bodies, than in electrics.

23. The resistance to derangement being connected with several important experiments, the results of which are considerably influenced by it, and in some instances wholly dependant upon it, we shall proceed to give a more detailed explanation of it. When a charged electric A, scheme 3, is brought to act by its deranging influence upon the atoms B, C, D, &c. there is, as we have before observed, a resistance on the part of each of these atoms, which must ultimately exhaust its force; and some distant atom in the series will consequently remain neutral. Now it is evident that this decay of force must be gradual, and that the atom B is exposed to a greater deranging force than C, and C greater than D, &c. Let us assume, then, that A presents a force of 120 on its positive and 80 on its negative side; and that in consequence of the resistance to derangement, the positive force of B is reduced to 119, and the force on its negative side increased to 82; the positive force of C is reduced to 117, and its negative force increased to 84: and that the different atoms D, E, &c. proceed in the same gradual approach to neutrality, until the two sides of the atom have equal forces. If we proceed with the series, and decrease the derangement in the same ratio, we shall find that the tenth atom, L, will

present a force of 101 on its positive, and 100 on its opposite surface, which being supposed to be the natural force of the atoms, the deranging force will cease; and any other atoms, M, N, &c., will remain neutral.

Scheme 3.

	A	B	C	D	E	F	G	H	I	K	L	M	N &c.
Positive force....	120	119	117	115	113	111	109	107	105	103	101	100	100
Negative force....	80	82	84	86	88	90	92	94	96	98	100	100	100
The charged electric	+o—	+o—	+o—	+o—	+o—	+o—	+o—	+o—	+o—	+o—	+o	o	o

Upon looking at the position of the different atoms, we find that the positive surface of B is exposed to the deranging force of the negative surface of A, which is 80, and that its negative surface is exposed to the force of C, which is only 117; whereas, to produce a corresponding derangement between the two surfaces of B, the latter force ought to be 120; there will consequently be a positive force on the surface of B, which its negative force cannot satisfy; the former requiring 19 atoms of light, and the latter being prepared to resign only 18; the other part, therefore, to bring the positive surface into a state of equilibrium with the different forces in action must be drawn from some other source. The same reasoning will apply to the atoms C, D, E, &c.; and even L, the last deranged atom in the series, will require a supply of light to complete the derangement of its positive surface, which its opposite, or neutral side will not be prepared to furnish.*

24. If, in the first place, we consider the series, represented in scheme 3, to be insulated from the commencement of the experiment, and consequently, that as no additional supply of light can be drawn from any other source, it must arrange its natural quantity agreeably to existing forces; the atom B, which is exposed to the deranging force of 100—80=20, will attract the light with much greater force than L, which is exposed to the deranging force of 100—98=2; and as the same may be said, though in a progressively less degree, of all the atoms in the series, the atoms B, C, &c. forming one half of the series,

* The ten atoms among which, for the purpose of illustration, we have distributed the deranging force of the electric, must be considered as ten points very distant from each other: and the light distributed, instead of being divided into twenty parts, must be divided into twice as many parts as there are atoms in the actual

will draw such a supply of light from G, H, &c., the other half, as will bring the whole into a state of equilibrium with the forces which are in action.

When this arrangement has taken place, if the atoms be divided in the centre and both parts continued in their insulated state, upon the removal of the electric A, one half, B C D E F, will be positive, and the other half, G H I K L, will be negative; or, if the series be divided into three parts, B C D E will be positive, F G will be neutral, and H I K L negative; F and G being at equal distances from the centre, the one during the deranging influence of the electric, will be as much overcharged as the other is undercharged, and upon its removal they will neutralize each other.

25. If, in the next place, we suppose the series to be connected with other conducting atoms, so as to form an endless or at least a very long conductor, such as it is when in communication with the ground, the additional light necessary to produce an equilibrium, will be supplied through this medium as a conductor; and if we then suppose the deranged part of the series to be insulated, and the electric, A, to be withdrawn, the whole will exhibit signs of a positive charge.

26. If, in the last place, instead of operating upon a series of atoms sufficient to exhaust the force of the electric, as in the previous experiments, we substitute a more limited number; B C D, &c., if supplied with light to bring the series into a state of equilibrium with the deranging force of A, through the medium of a conductor, will exhibit the same positive charge, when the electric is removed, as in the last experiment. If, on the other hand, a limited number of atoms, or a conductor of insufficient length to exhaust the force of the electric, be insulated, the effects will be precisely the same as in the first experiment; the part of the series connected with B will be positive, the middle neutre, and the other part negative.

If the positive force of A had been presented to B, the effects would have been equal in degree, but in an opposite order in all the experiments; and the same explanation will apply, with the requisite substitution of terms and reversal of consequences.

27. These theoretical deductions are illustrated and supported by the following experiments.

distance which is found to exhaust the electrical force. The numbers assumed in this, and other places, are merely to assist the description; and are not intended to convey the most distant idea of the actual proportions.

"Let a cylinder of metal, N P, (see fig. 117, Plate XIV,) of some length, with rounded ends, and furnished in different parts with pairs of suspended pith-balls, to serve as electroscopes, being previously insulated, be placed in the vicinity of an electrified globe of glass, E, taking care that it be not sufficiently near to receive any quantity of electricity by transference."

"We shall find that every pair of balls except those situated in a particular plane M *m*, about the middle of the cylinder, will immediately diverge, indicating the electrical states of the parts from which they are suspended. Those at either extremity of the body, *n*, *p*, diverge the most; and the divergence diminishes as we approach the middle plane beforementioned, at which the body is in the natural or neutral state. The position of this plane of neutrality, M *m*, varies according to the distance of the electric, and the relation which that distance bears to the length of the body itself. If we further examine the species of electricity residing in the different parts, we shall find it to be negative in all the parts nearer to the electric than the neutral planes, and positive in all those more remote. (Elect. 102.)

"It will be recollected that in the changes we have thus traced as the effects of induction, there has been no transfer of electricity from either of the bodies to the other; as is sufficiently proved by their taking place equally if a plate of glass be interposed. Another proof is afforded by the circumstance that the mere removal of the bodies to a distance from one another, is sufficient to restore each of them to their original state. The globe remains as positively electrified as before; the cylinder returns to its condition of perfect neutrality; nothing has been lost, and nothing gained on either side. The experiment may be repeated as often as we please, without any variation in the phenomena. But this would not be the case if the cylinder were divided in the middle, and one or both of the parts were removed separately, while they still remained under the influence of the globe. The return of the electric fluid from the positive to the negative end being thus prevented, each part will retain, after its separation, the electricity which had been induced upon it. The nearer portion will remain negative; the remoter portion positive. If the division had been in three parts, the middle part only would have been neutral. The experiment may be made by joining two or more conductors endwise, as shown in fig. 118, so that they may act as a single conductor when placed near to the electrified globe, and after induction has thus been produced, removing them separately, and examining their

electrical states. If E be positive, N will be found negative, P positive, and M neutral." (Elect. 105.)

These views are further illustrated by sections 106 and 107; but they are too long to be extracted, and we must refer the reader to the work itself, which is, no doubt, in the possession of most of those who feel an interest in the subject.

28. As we shall frequently have occasion to speak of the force to which all these phenomena may be traced, to avoid the necessity of repeating our explanation, we propose to call it the resistance to derangement. It enables bodies under the inductive influence of electrical surfaces, to appear neutral, when in possession of a quantity of light exceeding their natural proportion, if the electrical surface to which they are exposed be negative; and, also, when deficient of this proportion, if the deranging force be positive. This peculiar state of a body, in which it is overcharged or undercharged with fluid without any visible appearance of it, has been denominated by Biot, *disguised electricity.* (Elect. 107.)

29. Hitherto our remarks upon electrical induction have been in a great measure confined to conductors; but when the body exposed to the deranging influence of a charged electric is itself an electric, there is a resistance to its derangement arising from a cause to which we have not yet adverted. In conductors, an equilibrium is readily established by a transfer of light from one side of an atom to the opposite side of the same atom; and the light may be transmitted from atom to atom, by pressing forward and supplying the place of the atmospheres of the atoms in the line of transmission in succession; the light to be disposed of is pressed upon the atom A, which transfers an equal quantity from its own atmosphere to B; and from B the transfer is made in like manner to C D, &c. &c., until when the conductor is connected with the ground, it becomes generally diffused. A quantity of light belonging to the different atoms in the line of transmission, corresponding with the quantity transmitted, is thus thrown into the form of a current. But the transfer from one side of an atom, to the oppositeside of the same atom, and the transmission from atom to atom, are both denied to electrics, which have no means of producing an equilibrium except by a transferfrom the surface of one atom to the contiguous surface of the atom next in succession; beyond which the non-conducting properties of the electric prevent any communication. Thus, if we suppose A B C, &c., scheme 1, to be an electric, the interior atoms B C D E can only be charged by a transfer of light from the negative surface of F to the positive surface of E; from the negative surface of E to the positive surface of D; from

the negative surface of D to the positive surface of C; and from the negative surface of C to the positive surface of B; and these atoms can only be discharged by the reverse operations, (17). Hence the electrified globe E, fig. 117, Plate XIV, acts upon the conductor without any displacement of its own fluid from the reaction of this conductor thus brought into an electrical state. (Elect. 103. 104.)

30. If the atoms A B C, &c., scheme 1, have equal capacities, they may be fully charged upon this principle by taking light from the surface of A, marked — (negative), and adding it to the surface of F, marked + (positive), or by connecting these surfaces, by means of a conductor, while the body is under the influence of the deranging force. But electrics derive their peculiar properties from a want of this equal capacity between their contiguous atoms, the atom B not being prepared to receive the same quantity of light which A endeavours to dispose of; and as B can only act upon C by the force which it receives from A, the decline of the deranging force is much more rapid in a series, such as B C D, &c., scheme 3, supposing it to be an electrical body, than in a similar body formed of atoms which have equal capacities.

The difference of capacity in the atoms A and B, necessarily implies a difference of intensity; and this renders them incapable of transmitting a current, because the atmosphere of A must supply the place of the atmosphere of B, and B of C, &c., in succession. If, for example, we suppose the atoms B C D, &c., to have capacities alternately equal to, and less than A; that is, that B is equal to A; C, less; D, equal &c.; B will be exposed to a greater force from A, than it can convey to C; and as C can exert only the force it has received, its action upon D will be less than the action of A upon B; and less than it would have been if the atmospheres of the atoms B C D, &c., had been of uniform capacities; the force of derangement is therefore much sooner exhausted. (22).

31. But when atoms are charged, as they must be in non-conducting bodies, by a discharge from contiguous surfaces; the atom B will require a greater discharge to bring it into equilibrium with A, than C requires, or can receive, for a similar purpose, and B will exert a force to dispose of the superfluous light, which, when the action is continued for a sufficient time, will drive it to the opposite side of C; for the atoms even of electrics are not such absolute non-conductors as to wholly resist this transfer under the influence of a considerable force; and from C it will readily pass to D, the positive surface of which will by this means acquire a charge more nearly approaching to B; and ultimately the whole

series will be brought into a higher state of derangement than could have been produced without this transfer.

32. In our previous descriptions of the manner in which bodies are electrically charged, by a transfer of light from the surface of one atom to the contiguous surface of the next in succession, it might be expected that it would only be necessary to bring the opposite surfaces of such bodies into communication, in order to discharge them; but this is only strictly true when bodies are formed of atoms of uniform capacities; and are uniformly charged. In electrics, where the different forces in action produce the complicated arrangement we have endeavoured to describe (31), the discharge is never instantaneously completed; even in very thin electrics, such as the Leyden jar, it leaves what is usually called a residual charge (Electricity 123.); and in bodies of considerable depth, a very small part, if any, of the charge is removed by connecting their positive and negative surfaces; such bodies, like the magnet, and upon the same principle, having in some degree a permanent derangement. It is evident, from the manner in which these bodies acquire their charge (31), that the communication between the surfaces must be long continued to bring them into a completely neutral state.

33. The distinguishing phenomena exhibited by the electrophorus are readily accounted for upon the principles we have endeavoured to explain. (See Electricity 192).

When the electrical cake in this instrument has been properly excited, the different capacities of its atoms prevent its discharge; and it is in a state to exercise its deranging force upon other bodies without losing its own power (32). If when the cake has been negatively excited, the cover held by its insulating handle be placed upon it, the contrary or positive state will be induced in the cover on the surface in contact with the electric, and its capacity will be increased by the resistance of its atoms to derangement upon the principles we have already explained (23); but being insulated, this increased capacity cannot be satisfied, and it only leads to a different arrangement of its own light; the under surface in contact with the electric, being rendered positive, the middle neutre, and the upper surface negative (24). If, while in this state, the upper or negative surface be touched with the finger, or with any other conductor communicating with the ground, a spark will pass from the latter to the cover, to satisfy the increased capacity arising from its contact with a negative body. But when the plate is raised, provided it be held by its insulating handle, the action of the cake being withdrawn, the cover is found to be charged with positive

electricity (25. 26.). This operation may be repeated an indefinite number of times, since the electricity of the cake continues unimpaired during the process (18.). The sole of the electrophorus being in contact with the positive surface of the cake, for bodies in an electrical state whether under or over charged have positive and negative surfaces, as its capacity decreased (26.*)*; and, if insulated, a spark may be obtained from it, when the cake has been excited.

34. "If while placed on the cake the cover be touched with the finger, and at the same time the sole be touched with the thumb, a sensible shock will be felt in that part of the hand." This I am inclined to think, although I have not tried the experiments necessary to ascertain it, arises from the increased derangement of both sole and cover upon completing the circuit, a return to a natural state being prevented by their contact with the electric; and, if so, the current must be from the cover to the sole, or from the negative to the positive surface; upon the same principle that the current passes from the negative surface of the zinc to the positive surface of the copper, upon completing the circuit in a galvanic combination of these metals; the atoms of the bodies being in both cases more highly deranged, although they are thus put in possession of their natural quantity of light.

35. Ritter's secondary piles retain the charge which is given to them by the inductive influence of a voltaic battery, upon the same principle that permanence is given to the derangement of the cake of the electrophorus; the difference of capacity in the elements of which they are formed, preventing the transfer of light required to bring them to a natural state. (See Galvanism 93.)

36. Some of the most interesting appearances in electricity, arise from the attraction and repulsion of the surfaces of bodies in an excited state. The principles upon which these phenomena may be explained, being very simple, and having been already investigated, may be concisely stated.

Every atom of light repels every other atom of light; the comparative force of repulsion may therefore be ascertained, by multiplying the atoms in action upon one surface by the atoms in action upon the contiguous surface. Again, every atom of light in action, forming part of the atmospheres of two contiguous atoms of matter, attracts the matter of the

* 26 with a star, refers to the latter part of the section 26, beginning with, "If the positive surface of A had been presented to B, the effects" &c., and the section should be divided accordingly.

adjoining atom, as well as its own, and thus assists in drawing them together; the comparative force of attraction may therefore be ascertained by adding these atoms together.

37. When two atoms in a neutral state are placed in contact, there is no action either of attraction or repulsion; we may therefore conclude that the two forces balance each other. Let us suppose that the surfaces in this case present to each other a force of 100 atoms of light; the repulsive force between the atmospheres of two such atoms, and consequently between the atoms to which they are attached, will then be $100 \times 100 = 10{,}000$: and the force of attraction, which, as proved by the neutrality of both, is equal to this repulsive force, will be $100+100=200$. Let us further suppose that when the same two atoms are electrically deranged, the contiguous surfaces present 120 atoms of light on the positive surface and 80 atoms on the negative surface; the force of repulsion will in this case be $120 \times 80 = 9{,}600$, and the force of attraction $120+80=200$, as before; there will consequently be a balance in favour of attraction. This balance will increase with the increase of difference between the two surfaces. The distance from each other of the atoms of matter, the position of which within their respective atmospheres will be regulated by the forces brought into action, (12.) will be the same in both cases; for when they are in a neutral state they will be drawn together by $100+100=200$, and when deranged by $120+80=200$ atoms; the force arising from their mutual attraction will consequently be unaffected by the derangement of their atmospheres. We thus satisfactorily account for the attraction between surfaces in different electrical states.

38. When two positive or two negative surfaces are opposed to each other, the position of the atoms of matter, with regard to each other, is such as to leave no balance of force either of attraction or repulsion. When the positive surfaces are in contact, supposing each of them to be charged with 120 atoms of light, the atoms of matter are drawn together by a force of $120+120=240$ atoms; the atoms leave their central position in their respective atmospheres, in obedience to the greater force on their positive surfaces (12.), and this by bringing them nearer to each other, increases their reciprocal attraction for the atmospheres of each other; but to counteract this increased attraction we have the repulsive force of $120 \times 120 = 14{,}400$. When the negative surfaces are in contact, supposing each of them to be charged with 80 atoms of light, the atoms of matter are drawn together by a force of only $80+80=160$ atoms; they are consequently at a

greater distance from each other, and from the contiguous surfaces of light, than when in a natural state; and the force of attraction, produced by their reciprocal action, is only equal to the repulsion of $80 \times 80 = 6{,}400$.

39. The conclusion that the forces in these cases balance each other, is perfectly consistent with our theory, and we have strong evidence that it is correct; for if we admit that when positive surfaces are opposed to each other there is a balance in favour of repulsion, we must also admit that when negative surfaces are in a similar situation, there is a balance in favour of attraction; whereas experiment has decided that the force in both cases, from whatever cause it may arise, has the appearance of being repulsive.

40. If, however, we take into consideration the state and the connexion of the different surfaces, it will enable us to give an explanation free from difficulty. The most simple electrometer we have, is formed of two pith balls, supported by threads of equal lengths. When this electrometer is suspended from a body charged with electricity, whether positive or negative, the balls separate, and the degree of separation marks the strength of the charge.

41. This appearance of repulsion between the two balls, does not, according to our theory, arise from any action between their contiguous surfaces, these surfaces being in possession of opposing forces which balance each other; but the opposite or distant surfaces of the balls are in contact with the air, which, though an electric, is brought by the inductive action of these surfaces into a state of derangement of the opposite kind to that of the balls. If the balls are overcharged, their surfaces will be positive, and they will induce in the surrounding strata of air, negative surfaces directed towards their own centres, and positive surfaces directed towards the strata of air next in succession; which will be continued to more distant strata, until their forces are exhausted. When the balls are undercharged, the arrangement, and the effects of it, will be reversed.

42. While the balls are in contact these deranged strata of air cannot entirely surround them; and, consequently, any force which they may produce will act on one side without being counteracted by an equal force on the opposite side; the balls, therefore, will be separated, not by a repulsive force between themselves, but by the action of the contiguous strata of air, which present themselves in an opposite state of derangement, and consequently exhibit an attractive force. As the balls separate, the derangement will be extended to the air which will then occupy the space between them, and the

degree of separation would mark the extent of derangement in the surrounding atmosphere, if its tendency to separate the balls were not in some measure counteracted by their weight.

There is little or no action, in this case, between the balls and the distant strata of air; the force being communicated from stratum to stratum, acting upon each other in succession: the stratum A, for instance, which is supposed to be in contact with the ball, adheres to, or attracts it; the stratum A communicates the derangement to, and is in consequence attracted by another stratum B; and B communicates the derangement in like manner, and is attracted by C D E &c. in succession. When by their separation, the air is admitted between the balls, the different strata, A B C &c., surround them; and the attraction of the surrounding strata being rendered equal on all sides by their completion, there is no balance of force; the balls, consequently, must now be supported by the action of more distant strata, which are still deficient on the sides of the balls contiguous to each other.

43. The objection which has usually been urged against a single electric fluid, that it necessarily involves the condition of a mutual repulsion among the particles of ordinary matter (Electricity 233.), is entirely obviated in this explanation. Our theory supposes, that when the atoms of bodies are in a natural state, the repulsive force of the light connected with them, is exactly counteracted by the attraction of the matter of these atoms; and that as the two forces are obedient to the same laws, this is the case at all distances (6). It is the derangement of the natural state of bodies that leads to all the phenomena of electricity. It is, in fact, a very slight derangement of the two *immense* forces we have been speaking of, that produces all the phenomena which are the subjects of philosophical investigation; for while these forces are in equilibrium, nature is inactive.

44. Another objection has been urged, by Biot, against the hypothesis of a single fluid, on the ground that it implies an equal degree of attraction between the fluid and every species of matter (Electricity 240.). This objection, however, cannot apply to the present theory, the very foundation of which rests upon a difference of electrical force in the bodies which are the subjects of experiment (11.).

45. I have endeavoured in this paper, to apply the theory to such of the experiments usually introduced in treatises on electricity as appeared to me to present the greatest difficulties, and the most general character; but it applies with equal ease to the whole of these experiments, and I have only been prevented from pursuing the subject further, by supposing that

the reader himself will readily extend it to what has been omitted, and from a wish to avoid lengthening the paper unnecessarily.

LXXI. *On Lightning Conductors, particularly as applied to Vessels.* *By* Martyn Roberts, Esq.

Read before the Electrical Society of London, June 24th 1837.

As accidents, attended with loss of lives and property, are constantly occurring in our Royal Navy and Mercantile service, from the effects of lightning, notwithstanding the provision often made for their defence; I have been induced to turn my attention to the subject, as well in a philosophical, as in a nautical point of view, and I trust *by an examination of the causes,* and a *citation of a few effects* consequent upon them, to *solve the embarrassment* under which we now lie, and *to point out a means* whereby such disastrous consequences may be in future prevented.

The causes may be traced to two principal heads, namely, the form, and the position, of the conductor used at sea. The conductor most in use is a chain, each link of which becomes by the action of the saline moisture of the atmosphere highly oxidated, and as oxide of metal is a non-conductor of electricity, we have at every junction of one link with another, a solution of continuity in the chain regarded as a conductor of electricity, and therefore when the vessel is struck by lightning, every joint of this conductor becomes a point at which an explosion may take place, and where consequently the electric fluid may strike off in any direction whatever.

Its position, when the royal and top-gallant masts are struck, is often productive of danger and inconvenience, for the portion of chain not then stretched, (the spare part) is allowed to remain on deck, or towed overboard, either position being one of danger to the ship. The other form of conductor used is that invented by Mr. Snow Harris, with considerable ingenuity, but in my humble opinion by no means tending to diminish the danger. Mr. Snow Harris's plan is to let into the after part of the masts a strip of copper, of considerable surface, but of little thickness, (under the conviction that superficies, not content, conducts electricity) from which opinion I must beg to differ, as it is decidedly contrary to experiment; and if he made his strips thick enough to be efficient, they would materially injure that essential quality in masts, pliability.

While differing so widely from a gentleman for whose talents I have the greatest respect, I feel myself called upon to lay before the Society a few of the effects likely to result from an adoption of the system he advocates.

As I before mentioned, he proposes that a strip of copper be let into the after part of each portion of the mast, viz.: the royal, top-gallant, top, and lower mast, through the keel into the water.

Now, in the first place, as at every joint of the mast there must be a separation in the copper to allow the masts to be lowered, the same effects must be expected as have been condemned in the chain; and even supposing the lightning to pursue its course downwards over the copper strip, it appears to me highly dangerous to conduct such an immense accumulation of electric fluid as that in a lightning cloud, into the body of the vessel, close, generally speaking, to the powder maganine, or at all events among many substances that would produce awful effects from its action on them : the lateral explosion, of which I here speak, can easily be proved by experiment to take place even in the transmission of the feeble quantity of electricity generated by our machines, what great reason then have we to dread the enormous quantity of the fluid which will be conveyed into the hull of the ship? Indeed Mr. Harris himself gives an instance of most serious injury, arising to a sailor leaning against a mast through which the lightning was transmitted.

From what has been instanced, I feel confident that few will dissent from the position I wish to defend, *that a perfect metallic continuity of conduction from the mast head to the water, and also a transmission of the fluid through a channel far removed from the interior of the ship, is absolutely necessary for the protection of vessels from Thunder Storms.*

To attain this desideratum *under all circumstances,* I beg to propose the following plan.

Let conductors be made of a metallic rope, consisting of some hundreds of fine annealed copper wires, laid up as a common hemp rope; it will be pliable, may be rove through blocks, and traverse as well as any other rope. Let this rope be fixed to a copper point at the highest mast head, led down the after part of the mast, until it arrives at the lower mast head, and from thence led as a backstay to the outside of the ship, and there fastened to her copper sheathing. By this means a perfect metallic conducting channel is maintained for the lightning, from the highest point to the water, without interruption or contact with any thing that can possibly produce ill effects.

When the masts are struck, or half struck, the spare part of the conductor can be stopped up and down its standing part by a very simple means: thus let the part *c c c* fig. 126, Plate XV, that leads from the lower mast head to the water, be rove through two blocks, B B′; and let the lower block have a lanyard *l* attached, on hauling which, the conductor will in a manner be sheepshanked, and the lanyard can be belayed to the conductor itself. The small arrow shows the direction in which the conductor is finally led to the copper sheathing of the vessel.

MARTYN ROBERTS,
Bryn. y. Caerau,
Llanelly.

LXXII. *An experimental investigation of the influence of Electric Currents on Soft Iron, as regards the thickness of metal requisite for the full display of magnetic action: and how far thin pieces of iron are available for practical purposes. By* W. Sturgeon, *Lecturer on Experimental Philosophy, at the Honourable East India Company's Military Academy, Addiscombe, &c. &c.*

Read before the Electrical Society of London, August 5, 1837.

About some seventeen years ago a considerable degree of interest was excited, in the philosophical world, by some singular and unexpected facts which were discovered by Mr. Barlow, in his well-known series of experiments on the magnetism exhibited by soft iron when under the influence of terrestrial magnetic action only. The experiments of that justly celebrated philosopher, to which I shall have more particularly to allude in this Paper, are those by which the following curious fact was developed: viz. that the extent of magnetic action exhibited on a compass needle, by this class of ferreous bodies, depends, principally, on the *extent of surface* which they expose, and not on the mass, or quantity of iron which they contain. Notwithstanding, however, the generality of this law as regards the influence of surface, it was very natural to imagine that some subsidiary law must necessarily be in operation with reference to the *thickness* of metal absolutely requisite for the full development of the magnetic action due to any given extent of surface. This obvious inference was not likely to escape the attention of the able philosopher who was conducting the experiments. Mr. Barlow accordingly furnished himself with the necessary specimens, (both as regards *thickness* and *quality* of iron,)

for the complete investigation of this philosophical problem, the solution of which, whether regarded in a theoretical or practical point of view, appeared to be of the highest importance to the science of magnetism.

Mr. Barlow's experiments appear to have been conducted with great care, and through an extensive series of ferruginous specimens; and from the results which they afforded, it appeared that iron of about *one-tenth* of an inch in thickness* displays as much magnetic action on a compass needle as though it were of much greater substance; and another series of experiments subsequently conducted by Capt. Kater, were attended with similar results.

This important law, which governs what may conveniently be considered the *natural* display of the magnetism of soft iron, reduces the standard action of immense masses much below that which would have been exhibited by them, had it been proportional to their solid contents; and which would probably have amounted to an almost unmanageable extent on the steering compass, when emanating from the great number of heavy pieces of iron which now enter into the construction and equipment of large men of war. And on the other hand, this law places at our command an immense magnetic action from a comparatively small quantity of iron, by a mere extension of its surface into any required form.

By taking advantage of this law in combination with that which governs the action by *proximity*, Mr. Barlow was enabled to counteract the magnetic effects of all the iron on board of our largest ships of war, by the employment of a thin disc of that metal not more than twelve inches in diameter.

Notwithstanding the importance of the investigations I have now mentioned, nothing has hitherto appeared, of a similar character as regards the action of soft iron when under other influences than those of terrestrial magnetism. About some four years subsequent to the celebrated Œrsted having laid the foundation of electro-magnetism, I had the good fortune to discover that bars of soft iron, subjected to the influence of electric currents, display magnetic action in a very eminent degree, and to an extent far beyond that which iron usually exhibits by any other mode of excitation. I also showed that the magnetic force, thus brought into play, may be annihilated—reproduced—and its polar character reversed

* By referring to the table of "Experiments of Plates of Iron," that *chest iron* ·1384 of an inch thick gives the greatest deflection. Mr. Barlow concludes however, that the necessary thickness is "probably 1-20th of an inch."

with any velocity which the experimenter has at command for the requisite transition of the electric currents employed. These facts have been amply corroborated by subsequent experiments; and, latterly, their importance has become considerably enhanced by their being *those alone* on which the prevailing hope now rests, of bringing the astonishing agency of electro-magnetism into practice as a first mover of machinery; and as a motive force of general application. It is in the latter capacity, especially in the application of this force to purposes of locomotion, that the determination of the requisite thickness of soft iron for the full development of its magnetism when subjected to the influence of electric currents, becomes a consideration of the first importance; in order that the vehicle in which it is employed may be entirely free from an encumbrance not essential to the production of the force which the iron is susceptible of displaying in the shape best adapted to the construction of the engine; and which, whilst solid masses are employed in the construction of large engines, may possibly amount to an extent sufficient to neutralize a considerable portion of the power absolutely produced; and, perhaps, to completely extinguish the most ardent hopes, and frustrate every design of those who enter on this laudable pursuit.

It is now about nine years since my attention was first directed to this important branch of electro-magnetics; and, shortly afterwards, I instituted a series of experiments for a full investigation of the subject. The results of these experiments have never yet been published, and as nothing of the kind have hitherto appeared from any other quarter, they are yet as new to the scientific world as if of yesterday's discovery: and I hope of a sufficiently interesting character to offer to the notice of the Electrical Society.

The first point necessary to ascertain in this enquiry was simply this. Do *hollow* pieces of iron display magnetic action to the same extent as *solid* pieces of the same figure and dimensions, when both are submitted to the influence of similar electric currents? To decide this point, and others connected with the investigation, the following experiments were instituted.

A piece of musket barrel, about twelve inches long and about nine-tenths diameter, was enclosed in a spiral copper conducting wire No. 15, which was well covered with sewing silk. The convolutions of the spiral were placed as close to each other as the insulating silk would permit; and the spiral covered nearly the whole of the iron tube from one end to the other. A solid cylindrical bar of soft iron of a little smaller

diameter than the bore of tube, was also provided, for the purpose of being introduced to, or withdrawn from, the interior of the latter at pleasure. The tube was also movable in the coil, (which was formed on a pasteboard lining,) and could be taken out and replaced by other pieces of iron, whose effects might be found necessary to ascertain in the investigation. A magnetic needle having an agate cap, and supported on a fine steel point, placed in the centre of a graduated card, was also provided.

The axis of the spiral was placed at right angles to, and in the same horizontal plane with, the magnetic needle: having its nearest extremity opposite to the centre of that instrument and twelve inches distant from its pivot. The source of electric action was a single voltaic pair of copper and zinc, placed in a porcelain pint jar. The exciting liquid diluted nitrous acid.

When an electric current from this battery traversed the helix the needle divided to 3°. The iron tube was now placed in the spiral and the deviation of the needle increased to 30°. This ascertained, the solid iron cylinder was slowly introduced to the bore of the tube, and the deflections of the needle noted at every inch which the former advanced towards the needle, in the interior of the latter. The results are exhibited by the following table. When the solid bar was made to touch the furthest end of the tube the deflection was 40°.

When Introduced.	Deflection.	When Introduced.	Deflection.
1 Inch	45°	7 Inches......	40
2 Inches	46	8 "	37
3 "	47	9 "	35
4 "	45	10 "	33
5 "	44	11 "	32
6 "	43	12 "	30

These experiments show that the deflection increases for the first three inches of the solid bar's introduction, but no further: and that it again diminishes for every advance of the bar from that point; and when the whole is enveloped by the tube, the deflection is about the same as by the latter alone.*

* This experiment was shown to Mr. Christie, in the summer of 1830, in the Royal Arsenal, at the time that I was carrying on my experiments on the thermo-magnetism of simple metals; and magnetizing some of the largest pieces of iron ordnance, and iron globes, both solid and hollow, by the influence of electric currents. A ten inch bomb shell, which I fitted up for the lecture table, has been exhibited in the Adelaide Gallery of Practical Science, for the last five years, and remains a prominent piece of the electro-magnetic apparatus of that excellent institution.

There was something so exceedingly curious in the results of these experiments, especially in the loss of magnetic action as the solid bar advanced towards the needle, that I was induced to repeat them many times, but they were always attended with similar results. I next examined the magnetic action of the external extremity of the solid cylinder when introduced to various distances in the tube, but nothing very interesting was discovered, its polarity being of the opposite kind to that exhibited by the end of the tube nearest to the needle, as was expected.

Other pieces of iron of various shapes and magnitudes were made to touch the furthest end of the tube, whilst under the electro-magnetic influence, and in every case a considerable increase of deflection was observed: but in none was the deflection so great as when the solid cylinder was introduced about two or three inches into the bore of the tube. I have sometimes observed about two degrees greater deflection when the whole of the solid cylinder was introduced than when not present, but this is not always the case, and therefore does not interfere with the general results.

The next experiments were made with another piece of the same musket barrel, but, being nearer to the muzzle of the piece than the former, was of somewhat thinner metal. The thinner end of this tube was closed with a solid plug of iron, firmly welded to it, and reached about half an inch inside.

When this tube was placed in the electro-magnetic spiral, with its solid end nearest to the needle, the deflection was about 2° less than with the former open tube. Several trials were made by changing them frequently in the spiral whilst the same current was traversing it, and the mean difference was about 2°. The increase of deflection by introducing the solid iron cylinders, was similar to that shown by the former open tube, with this exception, when the solid cylinder was introduced as far as possible, or till it touched the inner end of the plug, the deflection was invariably about 3° greater than when the solid cylinder was not present. This effect I attributed to the thinness of the metal at that end of the tube, which was of very different dimensions on the opposite sides. On one side of the tube the iron was about $\frac{1}{12}$, and on the other not more than $\frac{1}{20}$ of an inch thick.

Another iron tube, of the same external dimensions as the former, but not more than $\frac{1}{30}$ of an inch thick of metal, was next placed in the helix. The needle deviated to 22°. When the solid iron cylinder was wholly introduced to the interior of this tube, the deviation increased to 32°; and the needle stood deflected to 30°, even when the solid cylinder was with-

drawn. This curious circumstance induced me to vary the experiment in several ways. I heated the tube to redness and destroyed every trace of local magnetic action, so that when held verticle, the *lower* extremity, whichever it might be, invariably exhibited the same kind of polarity. This treatment of the tube, however, though it lessened the quantity gained, and retained, did not entirely remove the latter effect. The tube still exhibited greater magnetic action after the solid cylinder was withdrawn than before it was introduced. Tapping the tube with a piece of wood, so as to agitate its particles, whilst under the influence of the current, increased its magnetic action so as to deflect the needle two or three degrees more than before such treatment; but by no method which I could think of, would it become so powerful as by the introduction and removal of the solid cylinder; and the effect was nearly the same whether the current was traversing the helix at the time the solid cylinder was introduced, or that the connexions were made afterwards. The tube, with its solid cylinder, was introduced to the interior of the helix at one and the same time, both whilst the current was moving through it, and before the battery connexions were made; but still the effects were of the same kind, and nearly to the same amount in all cases. It occurred to me eventually, that since the deflection was greatest whilst the whole of the solid cylinder was in the tube, the former had become as decidedly polar as the latter; and would consequently retain *some* polarity whilst any part of it was within the helix. Under these circumstances the operation of the interior pole of the solid cylinder, whilst being withdrawn from the tube, would be on the latter, similar to the operation of any other magnet upon a ferruginous body, whilst rubbed over its surface. It would indeed magnetize the tube: which now being subjected to an auxiliary influence conspiring with that of the current, would become more active than by the influence of the latter alone. This explanation seemed to be satisfactory enough as far as regarded the magnetizing of the tube; but was no reason for the latter retaining nearly the whole of the additional power exhibited whilst the solid cylinder was wholly in the interior. The mystery however seemed to be reduced to the following question.

Are electric currents, which alone magnetize a piece of soft iron only to a certain degree of power, capable of retaining any portion of an additional power conferred on the iron from any other momentary source of excitation?

To set this question at rest, the tube alone was placed in the helix, and the needle deviated to 25°. The proper pole of a steel magnet was now made to touch the furthest end of

the tube, and the needle deflected to 70°. When the permanent magnet was withdrawn the angle of deflection subsided to 35°: so that the tube, by this treatment, had required an additional power capable of pulling the needle from 25° to 35°, which it retained for ten minutes, the time of electric action after the magnet had been taken away. On disuniting the battery and helix, without removing any part of the apparatus, the needle, after a few oscillations, reposed in the meridian: proving that, although the iron tube might still retain some trace of permanent polarity, it was incapable of affecting the needle at the distance of twelve inches: and, consequently, the action left was infinitely small when compared to that which was retained by the electric current.

The appearance of this novel fact induced me to place the pieces of gun-barrel in the helix again, and subject them to the same treatment as the thin iron tube had been placed under. Both pieces retained a considerable quantity of the additional power, conferred on them by the permanent magnet, whilst they remained under the influence of the current, but lost all trace of it when the current was cut off.

A solid cylinder of iron, of the same dimensions as the hollow tubes, was next placed in the helix. The needle deviated to 28°. The permanent magnet was now brought to the furthest extremity of the iron, and the deviation increased to about 74°. When the magnet was withdrawn the angle of deflection became reduced to 32°. When the electric current was cut off, the needle soon reposed in the magnetic meridian.

Returning to the principal subject of investigation I next compared the electro-magnetic action on the tubes and solid cylinder of the same dimensions. The two pieces of gun barrel invariably gave greater deflections of the needle than those shown by the action of the solid piece of iron: but the thin tube gave smaller deflections.

Cylindrical rolls of sheet tin (tinned iron) gave greater deflections than the thin iron tube, but something less than either piece of the musket barrel. A bundle of iron wire* gave smaller angles of deflection than the thin tube, and a cylinder of copper gave no motion to the needle.

* A member of the society, on seeing my apparatus on the lecture table, observed that Mr. Payne, late manager of the Adelaide Gallery of Practical Science, had tried the magnetic effects of a bundle of iron wire in an electro-magnetic helix, but did not exactly remember the results.

I am well aware that a hollow keeper, or cross piece, has long been applied to the large horse-shoe magnet exhibited in the Gallery: a contrivance, which, I believe, was also Mr. Payne's.

From the phenomena developed by the preceding series of experiments it appears that iron tubes of a certain thickness of metal, perhaps about $\frac{1}{10}$ of an inch, are susceptible of displaying as much magnetic force as solid pieces of the same dimensions, when both are submitted to the influence of a *single** electric current proceeding from a single voltaic pair of copper and zinc. This important fact is analogous to that discovered by Mr. Barlow; and now places at our command an immense magnetic force with a comparatively small mass of iron; a force which may be increased to any required extent, and by a process the most simple that could be imagined. The principal encumbrance now removed, what can be the remaining impediment to the full completion of Electro-magnetic engines for any purposes they may be wanted? The old engines of solid iron have worked well; the new ones, with hollow iron, will work better.

We also discover, by these experiments, that electric currents are capable of retaining, in active play, a greater degree of magnetic action than that which themselves alone are capable of exciting. Hence electric currents exercise two kinds of magnetic power on soft iron: the *exciting* power, and the *retentive* power. The difference of these two powers may very conveniently be called the *adscititious* power or force, because it is that which the current displays in addition to the *exciting* force: or that which is required above the latter to complete its maximum of magnetic action.

With the exception of a few electro-magnets which I constructed for rotations, the experiments I have hitherto described were the only ones I made on hollow pieces of soft iron, until the account of Professor Henry's experiments with the spiral conductor reached this country.† Since that time, however, it is well known to the scientific world that I have been much engaged in investigations with that apparatus; and have varied its form, and also the fashion of the axle

* By a *single* electric current I mean a current traversing a single helix: for it is possible that when the current is multiplied by traversing more than one helix over the iron, that some other law may regulate the phenomena. This point I mean to investigate shortly.

† Since this paper was read I have constructed a hollow iron magnet, consisting of two pieces of gun-barrel for the two branches, which are united by a cross piece of solid iron, welded to one end of each piece. The other two ends of the branches are open, (open poles). It is not a horse-shoe magnet, but a rectangular one. It has four helices of copper wire round each branch. It does not answer very well for lifting, but the helix gives a good spark.

iron, in as many ways, perhaps, as any one pursuing the subject. The results of some of those investigations are already well known, but there are others still in reserve, which I may now be permitted to describe.

The helix which I now employ consists of two distinct wires, the one rather thick bell wire, the other very thin. The former is about 260 feet long and forms the inner, or lower coil. The thin wire forms the upper or outer coil, and is 1,300 feet long.* Both wires are covered with sealing-wax varnish, but no thread of any description. This covering has the advantage of permitting the convolutions to lie much closer together than when the wire is covered with thread; and the action becomes much increased in consequence. The reel or bobbin is of wood, two inches long inside the cheeks. The axis is hollow for the reception of a bar, or other piece of iron.

The coiling of the wire is proceeded with in the following manner. One end of the thick wire passes from inside to outside of one cheek of the bobbin, at the bottom of the bed. This end of the wire may be left of any required length for battery connexion. The wire is now coiled in close convolutions until it arrives at the other end of the bobbin. A strip of silk is now laid over the coil for insulation, and to prevent the wax being rubbed off by the next about to be placed above it. The coiling now proceeds back again to the first end of the bobbin. Now another strip of silk covers the second coil, and a third coil over that, and so on till the whole of the thick wire is nearly taken up, leaving only a few inches which passes through one cheek of the bobbin for connexion with the other end of the battery. Through this wire, and this alone, does the battery or primitive current run; and it is from this coil alone that the spark is shown.

One end of the thin wire is now soldered to the last convolution of the thick one, and a strip of silk laid over the last coil. This done, the coiling of the thin wire is performed in precisely the same manner as the thick one. When the whole is put on the bobbin, the coil part of the apparatus is complete. The process is exceedingly tedious.

When the shock is taken from this helix, one hand is connected with the outer end of the thin wire, and the other

* It is well known to the readers of these Annals, that Professor Callan employs two wires similar to those here described: but the coils used by that philosopher are very different to that I am now describing. Professor Callan employs *long* coils or iron horse-shoe magnets. Those which I employ are short and thick, with hollow axis.

hand with either of the ends of the thick one. If the other hand is connected with the lower end of the thick wire, the secondary current has to traverse that wire. When the hands are connected with the two ends of the thin wire, the secondary producing the shock runs through that wire only; and the effect is greater than by the other connexions.

The helix now described is fixed to the base-board A, A, of the instrument represented by fig. 125, Plate XV. One end of the *thick* coil wire is united by solder to the copper wire *z*, and the other end of the same wire to the copper wire *c*. The brass studs *d* and *e*, with their balls, are united to the two ends of the *thin* coil wire. A pillar P, fixed to the base-board, rises behind the helix, and supports the wheel *w*, and the axis *a*, with its pulley, which is behind the pillar. The battery connexions, with this instrument, are made by the copper wires *z* and *c*. The current flows from the battery through the wire *c*, to the lower extremity of the thick coil wire; and after traversing the whole length of that wire, arrives, by proper connexions, at the amalgamated copper disc *m*, supported on a pillar as seen in the figure. From the mercury in the disc *m*, the current proceeds along the bent wire *t t*, to the brass stud *s*, which is fastened to the upright P, and thence, by a conductor, to the wire *z*, and so to the zinc side of the battery.

One end of the bent wire *t t*, is furnished with a socket which fits the vertical part of the stud S pretty tight; the other end is finely pointed and amalgamated, and dips into the mercury at *m*, at which place the circuit can be opened and shut with great rapidity, by means of a lifting piece or cam, which is placed on the axis *a*, and which is made to revolve by the wheel and band, as seen in the figure. The revolving cam lifts the spring *t t* twice each revolution of the axis *a*.

The wheel *w*, *w*, is 6 inches diameter, and the pulley on the axis *a*, is one inch diameter; these being united by a band, causes the latter to revolve six times whilst the former revolves once. Therefore by turning the wheel at the rate of three revolutions in a second, the circuit is opened 36 times, and consequently 36 sparks are produced in that period. Instead of the copper disc *m*, I sometimes fix a small bottle, containing a portion of mercury, in its place. By this means, there is no scattering of the mercury, and the spark is seen in the bottle, whose reflection increases the light.

Another mode of opening and shutting the circuit rapidly is by means of a notched zinc disc D, which, when the lifting piece is removed from the axis *a*, and the wire *t t* from its stud *s*, is fixed vertically to the pillar *p* and concentric with the

axle *a*. On the latter is fixed a spring wire, whose furthest extremity presses against the notched part of the disc. This wire revolves with the axle, and the point which touches the zinc disc passes over the notch, and consequently the battery connexion is broken every time the revolving trigger arrives at a notch in the disc. The disc has 30 notches, which, multiplied by 6, the number of times the trigger revolves faster than the wheel, produces $30 \times 6 = 180$ interruptions of the circuit, and consequently as many secondary currents (with their shocks, if required,) for each revolution of the wheel. The wheel can be made to revolve, with ease, three times in a second. Hence, $180 \times 3 = 540$ shocks can be communicated in one second; or $540 \times 60 = 32,400$ in one minute. When the room is darkened, a circle of sparks appears on the face of the disc.

When shocks are to be produced:—If to the hands—the cylinders *r*, *r*, which are connected with the studs *d*, *e*, by means of wires, are to be grasped one by each hand; and the wheel put in motion, when either of the discharging parts is attached to the apparatus. When shocks are to be communicated to any other part of the body, the common medical directors with glass, or other insulating handles, must be used. The metallic stems of the directors to be connected with the studs *d*, *e*, by thin wire, in the same manner as to a common electrical machine. The balls must communicate, either directly or indirectly with the skin, by similar means as resorted to when galvanism is applied.

The shocks are pretty smart, and the sparks tolerably bright, from this instrument; but they are still more so when a cylindric bar of iron is placed in the axis of the coil; and the wheel turned at a moderate speed. If, however, the wheel be turned rapidly, especially when the notched disc with its spring trigger, is employed, the powers of the instrument instead of being augmented, by the iron axis, are absolutely diminished; and when the speed is very great, both sparks and shocks entirely disappear. When I first discovered this singular fact, I was led to suppose that it arose from an imperfect contact of the disc and trigger when the latter rotated with great rapidity: but the noise that was made by the one scraping over the other, convinced me that the cause was not a want of contact.

I now got an assistant to turn the wheel, whilst I slided the iron bar to and fro, in the axis of the coil; sometimes taking it wholly out, and again replacing it in the bobbin: the room being quite darkened. The brilliancy of the sparks underwent no change by any motion which I could give to the iron

bar whilst in the interior of the bobbin. Nor could they be made to re-appear, when the velocity of the wheel was great, so long as the bar remained within the bobbin. But when it was taken entirely out, the sparks invariably made their appearance.

I was now led to try the effect of my old servants, the iron tubes, in the interior of the bobbin; and I soon found that the shocks given, when a piece of the musket barrel was placed in the axis, were much stronger than any I had before experienced from the instrument. The other piece of gun-barrel was tried, and the shocks were stronger than with the solid bar; and they were still more so by employing the thin iron tube before mentioned. The sparks were also brighter than with the solid bar; and appeared with greater velocities of the wheel: but in all cases, they disappeared when the velocity was very great.

I next substituted a cylindrical roll of sheet tin (sheet iron tinned,) for the iron bar. The shocks were now increased to an astonishing degree, and, with great velocities of the wheel the sparks were much brighter than with any of the preceding pieces of iron in the axis of the bobbin. With a bundle of thin iron wire* in the axis of the bobbin they were quite as powerful, if not more so, than with the roll of tinned sheet iron.

I placed a bundle of iron wires in the axis of the roll of tinned iron, but could not discover any additional effect. I also rolled in a compact coil a whole sheet of the thinnest tin I could procure, which was double the extent of surface of the roll previously employed; but this when placed in the axis of the helix did not produce such strong shocks as the thicker tin plate. Bundles of narrow strips of tin plate gave very strong shocks.

Some of the phenomena developed by this series of experiments are obviously analogous to some of those developed by the former. The scroll of thin tin plate, although double the extent of surface to the thicker, did not produce such

* It is something remarkable, that Mr. Bachhoffner, who, about a fortnight before this paper was read, had got one of my coils; but without knowing anything of my experiments, discovered that a bundle of iron wires in the axis of the helix caused it to give a better shock than when a solid bar was employed. When Mr. Bachhoffner became acquainted with what I had done, he very politely expressed a wish not to publish an account of his experiments, which he had drawn up for the present number of the Annals. To this, however, I could not think of consenting, knowing that his discovery must have been made independently of any knowledge of mine.

powerful shocks as the latter; showing that a certain thickness of the iron is necessary for the development of a maximum effect; and I think it is very possible, though I have not had time to try, that a still thicker plate of tinned or any sheet iron, might tend to increase the power of the instrument, to some greater extent than any it has yet shown.

To understand how it happens that the iron increases the power of the instrument when the discharges are slow, but decreases it when they are made rapidly, requires two distinct investigations. The explanation of the former effect will be found in the principles of magnetic electricity already explained in this volume; but to explain the latter effect it will be necessary to call to our aid another principle in magnetics, which, hitherto, I have not named.

In all cases where a ferruginous body is enclosed in an electro-magnetic spiral, the magnetic lines of that body will be arranged in the opposite direction to the electro-magnetic lines of the inner surface of the spiral, which keeps them in play; and consequently in the *same* direction as the outer magnetic lines of the spiral, as may be understood by looking at fig. 73, Plate X. Now the phenomena exhibited by the machine being produced only at the time of the primitive circuit being opened, they are those of the *terminal* secondary current, and are the effects of a *collapsion*, of both the electro-magnetic lines, and of the magnetic lines belonging to the ferruginous body; which by operating in concert, give a series of exciting impressions, greater than either of them would do alone. The large curved line in fig. 73, is to represent the situation of those magnetic lines belonging to the iron which have distended to beyond the spires of the helix, and give exciting impressions by their collapsing motions.

To understand the cause of the *lessening* of the power of the helix, by opening and shutting the electric circuit with great rapidity, it will be necessary that we call to remembrance a well known fact which is observed whilst magnetizing a piece of iron, by the influence of electric currents. *Time* is required to produce a maximum of effect. And again, when the current is cut off, *time* is required for the iron to recover its neutrality. From the appearance of these circumstances we are led to suppose, that either the iron is a bad conductor of the magnetic matter, and impedes its motions; or that the latter, like all ponderable matter, is naturally and sensibly *inert*. Either principle alone would satisfy the conditions of the phenomena which I have named, and which have been long known, but it does not appear so obvious, that the *novel* phenomena which I have described, can be owing to an inferior

conductibility of the iron, independently of the operation of the other principle. I am led to suppose that there is a *magnetic inertia*; and that the magnetic matter is as prone to remain in its *last placed* condition, as any other species of matter whatever.

The disappearance of the sparks and shocks by the introduction of the iron to the axis of the helix, could not possibly arise from an absolute torpitude of the magnetic matter belonging to the bar; for in that case it would be perfectly neutral, and the phenomena would be displayed with the same precision as if no iron were present. That some peculiar counteracting force is in operation during the presence of the iron, is sufficiently obvious; and as we have no knowledge of any other than the magnetic, by which counter currents could be excited in the coil, it is allowable to infer, that in consequence of the *magnetic inertia*, in conjunction with the imperfect conductibility of the iron for magnetism, the polarizations and depolarizations of the bar are *not simultaneous* with the polarizations and depolarizations of the helix, but are invariably later; and when the *distentions* and *collapsions* of the electro-magnetic lines of the helix succeed one another in *rapid* alternations, the *opposite* motions, or the *collapsions* and *distentions* of the ferreous magnetic lines, are respectively taking place. Both systems of lines are in motion, though similar poles meet one another. Both systems give exciting impressions, but in directions to produce opposite electric currents. A feeble current will result from the excess of the one over the other. When the circuit is opened and shut very rapidly, all the phenomena of secondary currents entirely disappear, even when no iron is present: a fact not easily accounted for upon any other principle than either *magnetic* or *electric inertia*; or upon both.

I cannot close this paper without pointing out the advantages that may be derived by employing a helix such as I have described, with any contrivance for producing a rapid succession of shocks, as a medical electric apparatus. A cylinder of copper and zinc, which would enter a pint jar, if excited by salt and water only, will be a sufficient battery for the instrument to produce very smart shocks. The expense of keeping up the power is thus reduced to a mere insignificancy, and the first cost is trifling. A strong shock and bright spark are produced by this instrument, when the battery employed consists of a copper and zinc wire, No. 15, immersed 1-10th. of an inch deep into dilute nitrous acid placed in a watch glass.

POSTSCRIPT.

Since this paper was read before the Electrical Society, the instrument there described has been seen by several scientific gentlemen, both in London, Manchester, Preston, and in Liverpool, who have considered it as the most portable, efficient, and economical electrical machine ever yet offered to the notice of the medical practitioner.

At the request of several medical gentlemen, I have undertaken to furnish each of them with a machine of this description, which will be manufactured under my own superintendance; and if any other gentlemen should prefer the inventor, I shall have no objections to receive their orders, and supply them as fast as the workmen can get them completed. A discharging apparatus, very different to, and less liable to be out of order than, that described, will in future be adapted to the machine. Letters must be post paid.

LXXIII. *Remarks on the error of supposing that a communication with the Earth is necessary to the efficacy of Electric Machines. By* R. Hare, M. D., *Professor of Chemistry in the University of Pennsylvania.*

Some time since, in looking over a volume of Cavallo's Electricity, I was surprised to observe that in order to give the greater efficacy to an electric machine, he advises that the cushion, or negative poles, should be made to communicate advantageously with the earth. As the means of accomplishing this object he suggests a conducting communication "with moist ground, with a piece of water, or with the iron work of the water pump."

It appears from the following passage in Turner's Chemistry, a work generally of great merit, that the erroneous impression which gave rise to these suggestions, has been adopted by a more modern author. We find, page 77, American edition, the following allegation.

"The electricity which is so freely and so unceasingly evolved during the action of a good electrical machine, is derived from the great reservoir of electricity, the earth. This is obvious from the fact that if the whole apparatus is insulated, the evolution of electricity immediately ceases; but the supply is as constantly restored when the requisite communication is made with the ground. In the state of complete insulation, the glass and prime conductor are positive as usual, and the rubber is negatively excited; but as

the electricity then developed is derived solely from the machine itself, its quantity is exceedingly small. When the machine is used, therefore, the rubber is made to communicate with the earth. As soon as friction is begun, the glass becomes positive and the rubber negative: but as the latter communicates with the ground it instantly recovers the electricity which it had lost, and thus continues to supply the glass with an uninterrupted current. If the rubber is insulated, and the prime conductor communicates with the ground, the electricity of the former, and all conductors connected with it, is carried away into the earth and they are negatively electrified."

I conceive that the earth has never, of *necessity*, any association with the phenomena of the electric machine; of which the power is evidently dependent on the efficacy of the electric in transferring the fluid from the negative to the positive conductor. When the conductors are both insulated, they are brought into states of excitement as opposite as the power of the machine is at the time competent to produce. If, under these circumstances, with one end of a metallic rod, (terminating in a metallic ball, or other suitable enlargement, and held by means of an insulating handle,) we touch the negative conductor, while the ball is approximated to the positive conductor, sparks at least as long and as frequent will be obtained as when the negative conductor, or cushion, has the best possible communication with the earth. I conceive that any metallic surface, or surfaces, duly connected with either conductor, must become virtually a part of the conductor, and partake of its excitement. In this predicament, whilst receiving a charge, are the coatings of a Leyden jar, or an association of such jars in a battery. The effect of the machine is merely to transfer the fluid from one surface to another. After the conductor, and any jar, or battery, associated with them are charged, there is no more electricity on the surfaces than before; since whatever one has gained the other has lost.

If the impression of the learned professor were correct, how could a battery or a jar be charged, when both it and the machine are insulated from the earth? Yet experience shows that it is under these circumstances that a charge is most easily imparted. When the conductors are in a state of excitement, and both insulated, the one will of course be as much below that of the surrounding neutral medium, and of the great reservoir, as the other is above that standard. When we connect either conductor with the earth, it returns of course to the neutral state of the earth: but the difference

between the excitement of the conductors is sustained by the power of the machine to the same extent as before; hence the length and frequency of the sparks will not be found to be sensibly altered. It follows that when either of the conductors are made neutral by connexion with the earth, that the other will have its excitement as much above or below neutrality, as the sum of the difference between each of the two conductors and the terrestrial neutrality, when both are insulated. Thus supposing, that when insulated, the one conductor is relatively to terrestrial electricity minus ten, and that the positive conductor is plus ten; when the negative conductor alone is uninsulated, the positive will be plus twenty; when the latter is alone uninsulated, the former will be minus twenty.

It seems to be a common, though as I believe an erroneous, idea, that a spark changes its character with the conductor from which it appears to be taken; so that when produced by presenting a body to the positive conductor, it is considered as positive, and as negative when produced with the negative conductor in like manner.

I have already observed that any conducting surface in connexion with either conductor, must act as a part of that conductor. Approximating to the negative conductor, a body (a ball, for instance,) while in communication with the positive conductor, is really enlarging or elongating the surface of the latter, so that when the spark passes, it must still be from the positive to the negative pole; and *vice versa*, elongating the surfaces associated with the negative conductor, till sufficiently near the positive conductor to receive a spark, does not alter the character of the phenomenon. In each case, according to the theory of one fluid, a current passes from the positive to the negative pole; according to the doctrine of two fluids, two currents pass each other.

The cause of the difference observed in the sparks in the two cases is, that they are usually received from a small knob upon a big ball, or the hand; or some other body comparatively large.

Whenever the fluid is contracted into a small jet on the positive side, its projectile power is increased; while, under the opposite circumstances, its projectile force is lessened. This is the sole cause of the long forked erratic form, of what is called the positive spark: and the short stubbed appearance of what is called the negative spark. The whole difference may be effected in whatever situation the sparks may be taken, by causing a large and a small ball to exchange sides. When the surface on the positive side is so small as to condense the electric matter before it jumps, the projectile force

is greater, and as in the case of the jet pipe in hydraulics, there is a medium size at which the greatest projectile power is obtained. When the emitting surface is too large, the projectile force is lessened, and the spark consequently made shorter.

The following passage in Cavallo's Electricity is that alluded to above. See vol. i. page 184; London 1786.

"Sometimes the machine will not work well because the rubber is not sufficiently supplied with electric fluid; which happens when the table upon which the machine stands, and with which the chain of the rubber is connected, is very dry, and consequently in a bad conducting state. Even the floors and walls of the room are in very dry weather bad conductors, and they cannot supply the rubber sufficiently. In this case the best expedient is to connect the chain of the rubber, by means of a long wire, with some moist ground, a piece of water, or with the iron work of the water pump, by which means the rubber will be supplied with as much electric fluid as is required.

LXXIV. *Description of an Electrical Machine, with a plate four feet in diameter, so constructed as to be above the operator; also of a Battery Discharger employed therewith: and some Observations on the causes of the diversity in the length of the sparks, erroneously distinguished by the terms Positive and Negative. By* R. Hare, M. D., *Professor of Chemistry in the University of Pennsylvania.**

Fig. 119, Plate XIV, represents a machine with a plate four feet in diameter, which I have recently constructed so as to be permanently affixed to the canopy over the hearth of my lecture room.

This situation I have found convenient, even beyond my expectations, as the machine is always at hand, yet never in the way. In lecturing, with the aid of a machine on the same level with the lecturer, one of two inconveniences is inevitable. Either the machine will occasionally be between him and a portion of the audience, or he must be between a portion of the audience and the machine. Situated like that I am about to describe, a machine can neither hide the lecturer, nor be hidden by him. With all its power at his command, while kept in motion by an assistant, he has no part of it to reach or to handle

* Communicated by the Author.

besides the knob and sliding rod of the conductor, which are in the most convenient situation.

The object of this machine being to obtain a copious supply of electricity for experiments, in which such a supply is requisite, it was not deemed necessary to insulate the cushions and the axis, as in the electrical machine which I employ for experiments requiring insulation.*

The prime conductor is supported and insulated by means of wooden posts covered by stout bell glasses, so that the summits of the latter are between those of the posts and the inner surfaces of cups attached to the conductor. By these means the glass is subjected to pressure, but is liable to no strain. Such a support combines the advantages both of wood and glass. At C C are the collectors. R represents a sliding rod, which may be drawn out to such an extent as to be brought in contact with any apparatus placed under it upon the table.

In fact, the large rod, in which the rod R slides, may be slipped up to any elevation through the hole in the brass ball which sustains it.

Battery discharger for deflagrating wires. This apparatus is employed by me in lieu of Henley's universal discharger; being better adapted to my apparatus, and mode of operating. Two brass plates, S S, fig, 120, are secured to the pedestal by a screw bolt N, which passes through a hole made in each, near one extremity; the plates are thus allowed a circular motion about the bolt, so as to be set in one straight line, or in an any angle with each other. On one of the plates near the extremity not secured by the bolt, a brass socket is soldered, into which a glass column C is cemented, surmounted by a forceps. At the corresponding end of the other plate, there is a brass rod R, perpendicular to the plate, and parallel to the glass column. This rod is furnished with forceps. Between these forceps, and those at F, supported and insulated by the glass column C, a wire is stretched, which may be of various lengths, according to the angle which the plates S S make with each other. The pedestal should be metallic, or have a metallic plate at bottom, in communication with the external coating of the battery. This being accomplished, it is only necessary to charge the battery, without subsequently breaking the communication between the inner coat-

* See Silliman's American Journal of Science for 1828, Vol. VII.; London Phil. Mag. for 1823.

The machine alluded to in the above note has its plate horizontal. We expect that Professor Hare will soon furnish us with a description of this machine in its present improved state. Edit.

ing of the jars, and the prime conductor, by which the discharge is conveyed. In that case, touching the conductor is equivalent to a contact with the inner coatings of the jars, so far as electrical results are concerned. Hence, by causing one of the knobs of the discharger D, with glass handles, to be in contact with the insulated forceps F, and then approximating the other knob to the prime conductor B, the charge of the battery will pass through the wire W, as it cannot descend by the glass column, nor reach the operator through the glass handles. These should be longer than represented in the figure.

Long zigzag or erratic spark, contrasted with the short straight spark. "The cause of this difference between the lengths of the two electricities, we have no means of explaining." *Thomson's Work on Heat and Electricity.*

The object of fig. 121 is to represent the different forms and lengths of the electric spark, which take place between a large and a small ball, accordingly as they are made negative or positive. The long and zigzag, or erratic spark A takes place between a small ball attached to the positive pole, and a large one associated with the negative pole. The short straight spark B, is elicited under circumstances the reverse of those just mentioned. They are represented as simultaneous, but, with the same machine, can, of course, only be obtained in succession.

In no respect do the phenomena of mechanical elcctricity appear more favourable to the Franklinian theory, and more inexplicable, according to the doctrine of two fluids, than in the diversity of the electrical spark in passing between a small and a large metallic ball, according to the manner in which the balls are associated with the positive or negative poles of the machine. When the small ball is attached to the positive pole, the spark is long, comparatively narrow, and of a zigzag shape, such as lightning is often seen to assume: but when the situation of the balls is reversed, the spark is straight and thick, not one third as long, and nothing of a zigzag shape can be observed in it.

According to the Franklinian theory, when a body is more highly charged with the electricity than the adjoining bodies, the excess of the fluid is attracted by them, while it is inadequately repelled by the inferior quantity of the electric fluid, with which they are imbued. It follows that when a small globe is made positive in the neighbourhood of a large one, the excess of electric matter in the former, is attracted by all the negatively excited metal in the latter. When the small globe is made negative, the metal of which it consists attracts

all the electric matter in the large globe. Hence there is this difference in the two cases; the small globe being positive, a comparatively small *movable mass* of electric matter is attracted by a large immovable mass of metal: the small globe being made negative, a large *movable mass* of electric matter is attracted by a small immovable mass of metal. The charge being in both cases the effect of the same machine; the attractive power must be as great in one case as in the other. The forces by which the masses are actuated being therfore equal, it is quite reasonable that the greatest projectile power should be attained, when the small mass is movable. In that case, it will require less air to be removed in order to effect a passage.

There is an analogy between the difference which I suppose to exist in the case under consideration, and that which may be observed between the penetrating power of a rod which is blunt, and one which is pointed.

It remains to show why a large mass of electric matter will be discharged in a spark when there is sufficient proximity, although that electric matter be situated in the large globe, and attracted by the other, under circumstances which, as above stated, it would not pass without that proximity.

It must be evident that attraction increases, as the distance between the bodies which exercise it lessens. Of course the attraction of the small globe must always act more powerfully on those portions of the electric fluid, which occupy the nearest parts of the positively excited globe. But this difference of distance, and consequent diversity of attraction, increases as the globes are approximated. Thus, that portion of the electric fluid which sustains this pre-eminent attraction, will be accumulated into a conoid: the acuteness of which, and attraction causing the acuteness, increasing with the proximity, there will at least be sufficient projectile and penetrative power to break through the air, and thus open a passage for the whole quantity attracted by the small negatively excited globe.

When, by the process last described, the fluid is made to leap through a comparatively small interval, by the concentrated attraction exercised by a small negative ball upon the expanded surface of electric matter diffused through a large globe, the air does not become sufficiently condensed to resist it before it reaches its destination, and, of course, it cannot assume the erratic form which would arise from repeated changes in its course, as in the instance of the long spark.

Of the electrical brush. When the machine is in active operation, and the prime conductor insulated; from a small

knob attached to it, as at B, fig. 122, the electricity will be so sent off, as by the concomitant light to exhibit the form of a luminous brush, as represented in the figure at B. For the production of this phenomenon, it is necessary that the electric fluid shall be condensed into a small prominent mass, so as, agreeably to the preceding explanation, to have great penetrating power. This it cannot possess, when, with the same intensity in the generating power, a large ball is positively electrified. In that case, the electric column presents a front too broad to secure a passage through the surrounding non-conducting air. A small ball, negatively electrified, can only be productive of a diffuse attraction for the electricity in the atmospheric medium around it; so that it has less ability to create any penetrating power, than when acting upon the electricity in a comparatively large globular conductor, as in the preceding illustration. Hence, when the knob is on the negative pole, it may be productive of a luminous appearance in its immediate vicinity, where the electric matter, converging from the adjoining space, becomes sufficiently intense to be productive of light; but it does not produce the striking appearance of the luminous brush.

As, agreeably to Du Fay's theory, the knob, whether vitreously or resinously electrified, is surcharged with an electric fluid, the projectile power ought to be as great in the one case as the other, and the long spark and the brush should be producible in either case.

LXXV. *On a method of connecting electro-magnets so as to combine their electric powers, &c.; and on the application of Electro-magnetism to the working of machines. By the* Rev. N. J. Callan, *Professor of Natural Philosophy in the R. C. College, Maynooth.*

About four months ago, a method occurred to me, by which any number of electro-magnets might be connected together so as to obtain an electric current, having an intensity equal to the sum of the intensities of all the currents excited in the helices of all the magnets. I tested this method by experiment about a month ago, and found it successful. I coiled on two bars of iron, once round their entire length, a copper wire one-twelfth of an inch diameter, and left the ends of the wire on each bar, projecting. I then coiled about 150-feet of wire one-ninetieth of an inch diameter, over the thick wire. The ends of the thin wire on each bar were also left projecting.

The ends of the thick wire on each bar were successively connected with a pair of seven-inch plates, and the shock taken from the thin wire, of each magnet, by holding a pair of cylinders connected with their extremities of the wires. The shocks from the two magnets appeared to be equally strong. Afterwards the thin wires of the two magnets were united at one of their extremities so as to form one continuous wire, and the voltaic current was passed from the same pair of plates, through the thick wire coiled on both magnets. The shock given by the thin wire, in this case, (on breaking battery connexion) appeared to be twice as strong as the shocks given by either of the magnets, separately. Hence, if the thin wires coiled on any number of electro-magnets, be united together so as to form one continuous wire; and, if all the magnets be simultaneously magnetized by the voltaic battery, an electric current will be produced in the thin wire (at the moment of breaking battery communication) having an intensity equal or nearly equal to the sum of the intensities of the currents excited in each of the helices. Care must be taken to unite the thin wires in such a manner that the electric current excited in the helix of each magnet on breaking battery communication, may all flow in the same direction; otherwise, the current produced in the helix of one magnet may neutralize the current excited in the helix of another. From a small electro-magnet, an electric current equal in point of intensity to that of a battery containing 1,000 or 2,000 voltaic circles, may be readily obtained. Hence, by means of 100 electro-magnets, and a battery containing 10 or 12 large plates, we may produce an electric current equal in point of intensity to that of a battery containing 100,000 or 200,000 pairs of plates. An electric current of enormous intensity may be also obtained from a single magnet of very large size. But, I am strongly inclined to think that the electric power of 100 small magnets is considerably greater than that of a single magnet equal in size to the 100 magnets. First, because all long bars retain a considerable portion of the magnetic power imparted to them by the voltiac current. I have tried about fifteen bars of iron, varying in length from six to thirteen feet, and every one of them retained some of their magnetic power. Our large electro-magnet retains constantly a power capable of lifting about twenty pounds. Secondly, because long bars lose their magnetism, slowly and not in an instant.

Any number of electro-magnets for electrical purposes, may be united so as to increase those effects which depend on quantity of electricity. If the similar ends of all the thin wires coiled on the magnets be connected together, while the

similar ends of the thick wires belonging to the magnets are connected with the battery, as many electric currents will (at the moment of *making* and of *breaking* battery communication) be excited in the thin wires as there are magnets, and will pass through any body placed between the ends of these wires.

I have just made an electro-magnet which, with a single pair of 7 inch plates, gives (even at the moment of making battery communication) a shock as strong as the shock from about 30 or 40 pairs of plates.

In making electro-magnets which are to be connected for the purpose of obtaining increased electric intensity, care must be taken not to solder the thin to the thick wires of the magnets, and to leave both ends of the thin wires projecting.*

About four months ago I coiled on a cylinder of wood about 10 feet of covered copper wire $\frac{1}{8}$ of an inch thick. Over this wire, I coiled about 200 feet of very thin wire which was also covered. When the ends of the thick wire were connected with a pair of plates, a shock was received by holding in the hands a pair of cylinders connected with the ends of the thin wire. This shock was felt only at the moment of making battery communication, but not on breaking the communication. When the voltaic current was passed through the thick wire from a battery of 20 pairs of 2 feet plates, there was no shock on breaking communication, but, on making communication, the shock was very strong. The electric current which gave the shock was incapable of producing any effect on a very delicate galvanometer. When an iron bar was put into the helix formed of the thick wire, the shock felt on making communication was greatly increased, but was far weaker than the shock received on breaking communication.

In a paper published in the July number of the Annals, I stated that I was then employed in making an electro-magnetic engine to be worked by 26 electro-magnets. In the end of June, I tried the engine before it was completed, and found these two defects in it. The first was that some of the

* For the illustration of secondary currents at the lecture table, I find no method so simple and explanatory, as by having two *distinct coils*, each on its own bobbin. The battery coil of thick bell wire is on the smaller bobbin, and can be introduced to the interior of the thin wire coil at pleasure; the bobbin of the latter having a hollow axis sufficiently large for its admission. By this means it is obvious to every auditor, that the battery current has no communication with the outer coil. I sold a pair of these coils to Mr. Cottam, Secretary to the Manchester Mechanics' Institution, during my late lectures there. Edit.

magnets were connected by iron where they ought to have been connected by a substance not susceptible of magnetism; the second arose from the number of magnets which were divided into two sets, each containing 13 magnets. I found it imposible, in the plan which I adopted, to make an odd number of magnets work, and was therefore obliged to reduce the number of magnets to 24: or to two sets each containing 12 magnets. I also substituted a brass connexion between the magnets for the iron one. I then found that one small electro-magnet was capable of producing circular motion in a wheel which weighed about 100 pounds, and that six magnets were sufficient to give rapid motion to the wheel. The results of the experiments which I made, convinced me that electro-magnetism might be successfully applied to the working of machinery of every kind, and I resolved to get made an engine which would do the work of one horse or perhaps of two. This engine will contain 40 electro-magnets, and I expect that, with a battery containing 6 square feet of zinc, it will propel, at the rate of 7 or 8 miles an hour, a carriage which along with its load, will weigh 13 cwt. The engine will be ready for work in the end of this month or in the beginning of the next. By calculations founded on experiment, I have been led to the following conclusions. First, that an electro-magnetic engine as powerful as any of the steam engines on the Kingstown Railway, may be constructed for the sum of £250; secondly, that the weight of such an engine will not exceed two tons; thirdly, that the annual expense of working and repairing it will not be more than £300. If my calculations be correct, the expense of propelling the railway carriages by electro-magnetism, will be scarcely one fourth of the cost of steam. I have found that the first cost of an electro-magnetic engine, and the expense of working and repairing it, increase only as the square root of the power of the engine. Thus, the first cost of an engine of 100 horse power, and the expense of working it will be very little more than ten times the cost of an engine of 1 horse power. A battery containing 10 square feet of zinc will work an engine 100 times as powerful as that which requires only one square foot of zinc.

N. J. CALLAN.

Maynooth College,
September 11th, 1837.

LXXVI. *The Magnetic Balance.* By R. W. Fox, Esq.

A front view of this instrument is given at fig. 117, Plate XIV. The beam is shown at *a, b,* having an axis passing

through its centre of gravity, which is terminated by fine pivots working in jewelled holes.

This beam is itself a magnet, and its position is governed and adjusted by the repulsive action of two other magnets in the lower part of the instrument, situated as shown by the dotted lines at *c* and *d*; they turn with the axis *a* and *d*, and may be adjusted to any angle by the graduated circles.

The scale pans *e* and *f* are formed of slight silver foil and suspended from the beam by single fibres of unspun silk; they are, together with the beam, separated from the other parts of the instrument by slight partitions, to avoid the effect of currents of air; and the weights and materials are put in at the sides, where there are sliding glass doors, which are not shown in the figure. The mode of reading the indications of the instrument, by means of the double scale, is exactly similar to that used in Mr. Fox's dipping needle deflector:* in fact, the present is a new application of the principles of that instrument, and the experiments tried with it prove it to be equally successful. The advantages it possesses over the ordinary balance are extreme sensibility, it being capable of indicating much less than the ten thousandth part of a grain; the facility with which its indications are obtained; and the comparatively low price at which it can be rendered.

To adjust the instrument for use, it must be carefully levelled by the screws, two of which are shown at *g* and *h*; the beam is then brought to the zero points by turning the magnets, and when ascertained to be correct, it may be kept in this position by partly drawing up the arms *p q*, by the screw *r*, to prevent too much vibration while the weights and materials are being introduced. This done, the sliding doors closed, and the screw *t*, carefully released; the beam immediately takes its position, which is rendered exceedingly accurate by gentle friction applied at the point of a brass pin at the back, showing the difference of the materials and weights, by the small space passed over by the points of the needle: and as its value should be previously determined by experiment, the weight would be known; or, if preferred, weights may be added till the beam becomes perfectly horizontal. It is evident that this balance will afford great facilities for taking the specific gravity of minute bodies with extreme accuracy; and for this purpose the scale pan *h*, fig. 118, suspended by a fine wire, may be substituted, and a glass of distilled water placed in the drawer to receive it.

This instrument is manufactured by T. B. Jordan, of Falmouth.

* This instrument will be described in a future number. Edit.

LXXVII. *A letter to* W. Sturgeon, Esq.

Dear Sir,

I have lately been employed in experimenting with the compound helix which was kindly supplied me by yourself; and have devised the following instrument for making and breaking contact, which, from its convenience and portability, may, I think, prove interesting to some of your readers.

A is a mahogany box containing the helix: and here permit me to observe, that if a bundle of insulated iron wires are employed instead of the iron bar, the power of the instrument is increased at least twofold. The wire I usually employ is the common covered bonnet wire. I cannot detect any difference in the force or powers of the shocks by using one large piece doubled up, or in a series of small pieces, (I give preference, however to the latter), but the increase of power of the wires over the bar requires only a trial to be convinced of the fact. I mention the above circumstance, because I think it may be successfully applied to the electro-magnet, and, perhaps, to the magnetic-electrical machine.

B the helix; F the bundles of wires; *f g* are the terminals of the helix, the ends of which dip into the mercury cups 3 and 2.

C is the galvanic battery for transmitting a current through the helix; *h*, *i*, the poles terminating in the cups 1 and 2.

G is the top of the box on which stands the instrument for making and breaking contact, to which I have given the name of electro-magnetic; *o*, *o*, are two brass pillars, the ends of which terminate in the mercury cups 1 and 3.

E is a steel spring; D a ratchet wheel upon which the spring rests. By mere inspection it is evident that as the wheel rotates, contact is broken every time the spring leaves the tooth upon which it rests. The wheel has 36 teeth, and when made to rotate 120 times in a minute, which by a series of experiments I find gives the maximum of power, we have, you see, 4,320 distinct shocks following each other with great rapidity. 4 and 5 are two brass screws into which may be placed the conducting wires and tubes *j*, *k*, for receiving the shock.*

* To prevent mis-representation, we beg to state that Mr. Bachhoffner's method of opening and shutting the battery circuit is, as far as we are aware, perfectly original; his paper was put into our hands immediately after the business of the meeting of the Electrical Society of August 5th was concluded. Edit.

H, H, are two extra conductors, communicating with the thin wire, and consequently entirely without the battery current. The shock, when received from them, is the result of the secondary current only, and will be found to give shocks of greater power.

With this arrangement the helix becomes a very formidable instrument; the rapidity with which the shocks follow each other, together with their power, even with a very small voltaic arrangement, is so insupportable, that anything like grasping the conducting tubes with the hands moistened is out of the question. It is in fact impossible from the excruciating agony which they communicate.

With a Wollaston's battery of ten pairs of plates, the instrument far exceeds in power, in my estimation, so far as shocks are concerned, the largest battery which has ever yet been constructed. In my next I intend to send you an account of its chemical and igniting power, if you should consider them of sufficient importance, and remain,

Your's faithfully,
GEORGE H. BACHHOFFNER.

13, *Aberdeen Place,*
Maida Hill.

103, Newgate Street,
July 23d. 1837.

LXXVIII. *To* W. Sturgeon, Esq.

Sir,

I was lately experimenting with Dr. Ritchie's rotating magnet, substituting a horse-shoe of soft iron bound with helices of copper wire, in the place of a permanent magnet. While the bar was revolving, and it revolved very rapidly, I observed that one of the wires eminating from a Mullins's sustaining battery, rested merely on the edge of the wooden cup, instead of dipping into the mercury, so as to render the circuit complete; I was somewhat surprised, and immediately removed the bar, to apply a delicately poised magnetic needle, and found that the terminations of the horse-shoe attracted either pole indiscriminately, which led me to believe that the rotation in this instance was not caused by attractive and repulsive forces. I then had a bar of soft iron, bent in the usual form, but not covered with wire, and connected the revolving bar with a battery, when it immediately commenced its revolutions, and continued to rotate in the same direction, as might be expected, when the poles were reversed. What seemed curious was, that it rotated in either direction; but it

had a tendency when stopped to begin again in the same, unless urged to the contrary by a few revolutions. Instead then of two attractive and two repulsive forces being brought into operation in this experiment, as in the case when a permanent or voltaic magnet is used, there are but two attractive forces; for the terminations of the soft iron cannot be rendered repulsive in any manner, by means of the revolving bar, because it is from that alone it receives any supply of magnetism, and magnetism always induces the opposite states.*

I am, Sir, yours very respectfully,
JAMES C. COOMBE.

LXXIX. *A Letter to* W. Sturgeon, Esq.

My dear Sir,

Soon after I received the second number of your Annals, I made one of Mr. Kemp's voltaic batteries, the single one last described in that number, and after several trials could not produce the slightest magnetic action; a few days ago I resolved to give it another one, and was still unable to produce any effect. After a little reflection I placed a plate of thin rolled zinc on the surface of the fluid amalgam of mercury, which of course floated on it. On replacing the copper plate I found it in action: after a few minutes I took the copper plate off and was surprised to find the zinc amalgamated on the upper side which had not touched the murcury, and which must therefore have penetrated through it. I then removed the plate of zinc and put the copper plate again on, and found the battery then in action notwithstanding the zinc plate had been removed; and what is remarkable the action has continued ever since, which at the time I am now writing is upwards of forty-eight hours. The helix I used was made of copper bell wire, and not more than twenty-one rings. The piece of soft iron wire enclosed in it, one-eighth of an inch diameter, and sustains the key of a tea caddy lock, the diameter of the wooden box or cup four inches, internal measure.

There are two objections to the construction of this battery; one is the quantity of mercury required to cover the bot-

* Since this paper has been put in type, Mr. Coombe has discovered that his rotating piece performs well by the action of terrestrial magnetism, independently of any iron being present. This is what might have been expected, and is a very interesting variation of the experiment. Edit.

tom, and also on account of the acid liquor penetrating through the pores of the wood. Mine is made of hard beech. I have in my mind a plan to obviate both these, which when I have tried I will send you the result. If you think proper to insert this in your Annals, it may possibly attract the attention of your numerous readers.

I am, yours truly,
J. HARPER.

Oxford, Sept. 18, 1837.

LXXX. *Description of an arrangement of a series of the Sustaining Voltaic batteries, so as to obtain quantity or intensity. Also of an apparatus to give shocks by a single voltaic pair.* By E. M. CLARKE, *Magnetical Instrument Maker to the Royal University of Christiana, Norway,* 11, *Lowther Arcade, London.*

This arrangement consists of a number of sustaining voltaic batteries put in a double row in a deal case, having a partition length-wise between them; on this is fixed a set of circular blocks so as to fill the spaces that are left between the jars that contain the zinc and copper cylinders. The blocks have four holes drilled in them to contain mercury into which the conducting wires from the batteries dip. Fig. 128, Plate XVI, *a*, the box, having a cover that slips down on it; *b*, the porcelain jars that contain the sustaining batteries. *c* and *c'*. Fig. 128, and 129, the partition on which the blocks *d* and *d'* are fixed, Fig. 129 shows the arrangement of the conducting wires, those from the zincs being ranged in the line *e*, and the coppers in line *f*. The intensity conductor, fig. 130, consists of a slip of mahogany having pieces of copper wire passing through, and so arranged that each separate bent piece of copper wire will connect a zinc and copper cylinder alternately throughout the entire series. Any apparatus is readily connected by means of the binding screws at *g*, where pieces of wire are held in good contact with the battery, and brought into the mercury cups of the decomposition of water apparatus *h*. A battery thus arranged consisting of ten jars holding three pints each, has a power of decomposing water truly surprising, yet not capable of giving a shock of the most trifling description.

Fig. 131, the quantity conductors, being two rods of brass having pieces of copper wire soldered in and at proper distances, so as to drop into the mercury in the blocks *d*, it will be readily understood that by this arrangement, as both conductors are separate, one will connect all the copper cylinders,

the other all the zincs: the binding screws at each end afford the same facility of connexion as the intensity conductor. The apparatus represented in the figure as being in connexion with these conductors, is for giving shocks from a single voltaic pair, and consists of a hollow reel of box-wood, within which is a cylinder of soft iron. A coil of insulated copper wire is put on the reel on the same principle as described by Professor Callan, in the 5th. No. p. 376 of these Annals. *k*, *l*, a pair of conductors in connection with the fine wire coil. I use a wire for the secondary current, of $\frac{1}{58}$ of an inch diameter which I find gives a more powerful shock than a wire of $\frac{1}{40}$, as used by the learned Professor. On grasping the conductors in the hands and drawing the wire which projects from *k* along the surface of a steel file, a succession of violent shocks will be received, accompanied with brilliant scintilations of the burned steel; this arrangement answers very well for illustrating the effects of secondary currents giving shocks, but fig. 132 represents an apparatus for applying those shocks medicinally, and is an improvement* on an apparatus which the editor of these Annals showed me. *m* a cylinder of iron passing through the sides of the mahogany stand. *n* a coil of wire of the same description as already alluded to, the thick magnetizing coil being in connexion with the voltaic battery *o*. A pair of sponge directors are in connexion with the secondary coil, and as represented in the figure, a succession of shocks are passing from the knee to the

* It must at all times be gratifying to philosophers to observe that their discoveries and inventions are of sufficient importance to demand the attention of others, and to inspire, in instrument makers, a spirit of emulation to excel in their attempts to improve even the most trifling mechanical arrangement. On the other hand, it must be allowed that there is a respect and gratitude due to philosophers, which those who profit by their discoveries are but too seldom disposed to acknowledge. Notwithstanding the trivial alterations he may think proper to make, the fame of the instrument maker could not possibly suffer by a candid acknowledgment of the source of his information, on any point whatever, relative to the original invention: and the *least* degree of courtesy that could be expected from him would be to permit the discoverer, or inventor, to have a priority of publication. Mr. Clarke will observe, however, that we have not hesitated to give publicity to his paper without the slightest alteration, but it is our duty to state, that Mr. Clarke's method of opening and shutting the battery circuit is similar to that employed by Mr. Barker. See fig. 42, plate VI: and that the original employment of the spur-wheel and mercury for this purpose was by Mr. Page. See the postscript to that gentleman's paper, page 293 of this volume. EDIT.

toe. The peculiarity in this arrangement is the method of breaking the connexion with the voltaic battery, and is as follows: *r* a copper star which is made to rotate in the glass vessel *s* which contains a little mercury, by means of a grooved multiplying wheel *t*, having a band passing over a smaller one *v*, which is attached to the spindle that carries *r*, a point of which only touches the surface of the mercury, each point leaving before the other enters, by which means a succession of some thousand shocks may be given in a minute. The mercury is effectually prevented spilling about and is unaccompanied with any disagreeable noise. This instrument also decomposes water; in short performs all the different experiments that require an intensity arrangement. The quantity experiments that are performed by it depend more on the action of the voltaic battery than any assistance which this instrument gives.

LXXXI. *First Report of the Committee of the London Electrical Society.*

At a General Meeting of the Electrical Society held Saturday, August 5, 1837, the following Resolution was unanimously agreed to;—

Resolved. The weeekly meetings of the Electrical Society having been adjourned from the 12th. day of August, until Saturday the 7th. day of October, 1837, the Committee are requested to prepare and print a Report for circulation; and, in order that the general interests of the Society may not suffer, the Committee are further requested to meet from time to time during the recess and act in every instance as if the general meetings were continued; it being understood that a Report of the Committee's proceedings is to be submitted to the Society at the first general Meeting.

The Committee have now, in pursuance of the above Resolution, to offer to the Society the following

REPORT.

The Committee have great pleasure in announcing to the members of the Electrical Society, that the object for which the Society was formed, as stated in the resolution of 16th. May, 1837, will be fully carried into effect; at the same time, the Committee claim from every member his individual cooperation, not only by procuring an additional number of subscribers but also by diffusing among his scientific friends the knowledge of the existence of the Society, its rules, and

objects; as the Committee feel assured that the views of the Society have only to be made public to insure its ultimate success.

It was originally the intention of the Committee to make a selection from the numerous papers that have been read at the weekly meetings of the Society, in order that they might be printed and circulated among the members. The Committee, however, for the present, defer the publication, particularly as the weekly meetings have been attended by most of the resident members, who have had an opportunity of hearing the contents of the papers alluded to. The Committee are the more induced to delay the publication from some of the members having intimated their intention of presenting papers after the recess, comprising new but important facts in Electrical Science.

The following list of papers, already read at the weekly evening meetings, is printed for the information of those members who had not an opportunity of attending; and the Committee, with great pleasure, refer to the same as a proof of the spirit and energy with which those meetings have hitherto been conducted. Some of the papers will, by the liberality of the Editor of the "*Annals of Electricity*", appear, from time to time, in that work.

Read June 10. On the same cause under different circumstances, producing the varied phenomena of the different sciences of Electricity, Galvanism, and Magnetism. This paper was illustrated with many diagrams.

On the principle and character of a new Galvanometer.

24. On Lightning Conductors, particularly as applied to vessels.

On the theories of Franklin and Du Fay, as regards single and double fluids, developed by friction or electric machines.

On secondary electric currents and on their influence in certain arrangements of apparatus.

Read July 1. On the effects of peculiar electrical states of the atmosphere on the functions of animals and plants, and an attempt to explain the "*modus operandi*" of the electrical fluid in producing epidemic diseases.

Extract of a letter from Lieut. Morrison, R. N., dated Cheltenham, June, 1837, describing an instrument which he terms a Magnet Electrometer; it not only indicates the kind but also the degree of Atmospheric Electricity. Lieut. Morrison has intimated his intention of sending the Society the result of observations made by him with this instrument.

8. On atmospheric electricity. This paper was accompanied by a table containing a series of experiments made with the electrical kite, by Mr. Swaim, of Philadelphia.

On a new experiment with a magnetic battery, by which a continuous line of sparks can be produced.

15. On the inconsistencies of some of the present views respecting electricity. The author of this paper is at present carrying on a series of experiments to demonstrate his views, which will be laid before the Society at an early meeting.

22. On the effect of peculiar electrical states of the atmosphere, &c. (Second paper).

29. An original letter, dated November, 1822, from Andrew Crosse, Esq. to Mr. K. Spencer, describing the mode in which he then insulated his atmospheric apparatus.

August 5. A paper entitled "An Experimental Investigation of the influence of electric currents on soft iron, as regards the thickness of the metal necessary for the full display of its magnetic action, and how far thin pieces of iron are available for practical purposes."

12. Extract of a letter from Andrew Crosse, Esq. to W. Leithead, Esq. dated Broomfield, near Taunton, August 4, 1837, describing some interesting results obtained by him in producing Crystals by transferring the electric energy from the zinc and copper plates to other substances, not metallic, in contact with them.

On a new mode of increasing the power of the shock from a coil of wire when acting upon a voltaic current.

On the commonly received opinion that two electric currents can pass along a central wire in opposite directions. This paper is accompanied with a diagram showing the action of the electric currents on the magnetic needle.

At the meeting of June 24, the Rev. Mr. Shillibeer, of Oundle, Northamptonshire, a non-resident member, stated that he had given instructions for a series of his Voltaic Batteries to be prepared, and which he hoped the Society would favour him by accepting.

The thanks of the Society have been voted to Mr. Shillibeer for his donation: as also to Mr. T. Pollock for his work, entitled, "An Attempt to explain the Phenomena of Heat, Electricity, Galvanism, Magnetism, Gravitation, and Light, on the assumption of one cause or universal principle.

At the Meeting of the 22d July, the Chairman stated that if any Member was desirous of carrying on a series of experiments, he would, on mentioning his intention, and describing

the nature and object of the proposed experiments in writing to the Committee, be immediately afforded the necessary facilities.

The Committee, in pursuance of the instructions of the Society, continue to hold their Meetings during the recess, and should any Member require further information he will please address a letter to the Honorary Secretary, who will communicate the same to the Committee.

The Committee have further to announce that in consequence of the increased number of Members and Visitors attending the weekly Meetings, they have been compelled to seek a larger room for their accommodation. The Council of the Society for the Illustration and Encouragement of Practical Science have obligingly permitted the future Meetings to be held in their Lecture Room; in addition to which the Council have liberally offered the Society the use of their Electrical Apparatus. The Committee cannot but point out to the Members the advantages likely to accrue to the Society from the increased accommodation thus afforded to Members as well as Visitors, and fully rely on the active exertions of the Members, both resident and non-resident, in carrying out the objects for which the Society was originally formed.

The general Weekly Meetings will, agreeably to the 5th rule, be resumed on Saturday, the 7th October, at 7 o'Clock, notice of which will be sent to each resident Member: in the mean time the Committee will be happy to receive any communications through the Secretary.

WILLIAM LEITHEAD, *Secretary.*

London, Sept, 4, 1837.

The following extract taken from the Rules and Regulations unanimously agreed to at the Meeting of 20th May, 1837, the Committee have deemed it advisable to reprint.

RULES.

The Society for the present to consist of Resident and non-Resident Members.

The Subscription of a resident member (or one residing within 20 miles of London) to be for the first year *Two Guineas,* and that of a non-resident *One Guinea.*

Visitors may be introduced by any of the Members and will be permitted to join in the discussion.

The Society to meet every Saturday Evening, at 7 o'clock, for the purpose of conversation, and at 8 o'clock the Chair shall be taken. The Chairman being elected by the Members present.

ORDER OF BUSINESS.

Immediately on the Chair being taken, the minutes of the previous meeting are to be read, and the names of any Members subsequently registered, announced. The Secretary shall then read such papers as have been presented to the Society during the past week. After each paper has been read, a discussion may take place, should the members present consider it expedient.

All donations of money, books, or apparatus, to be entered in the minutes of the meeting next after they have been received, when they will be duly acknowledged.

Every paper communicated to the Society, in order to be read at its meetings, will be deemed the property of the Society; unless any engagement to the contrary shall have been stipulated by the Author.

LXXXII. *Miscellaneous Articles.*

To the Editor of the Annals of Electricity, &c.

Sir,

All the writers of the present day have led us to believe that Mr. Faraday invented the apparatus called the voltameter, and on reference to his papers read before the Royal Society (735), &c., it will be found that he there claims it as his own. Judge then of my surprise, in reading the following passage in Donovan's Galvanism, and how nearly the opinions there entertained of its merits as a measurer of voltaic electricity, coincide with those expressed by you in the last number of the Annals.

"He, (Robertson) also describes an instrument, the principle of which has been since frequently used for measuring the decomposing energy of any galvanic series. It consists of a tube of glass filled with water, and containing a wire at each end which comes very near the other within. The tube stands vertically, and is graduated at its upper end, so that the water is resolved into gases, the quantity of which being ascertained by the scale, gives, when compared with the time, the energy of the series. Various other instruments bearing this name have been since contrived; but they are really electrometers, and do not measure the decomposing power of the pile."

The priority of invention is so obvious, that it needs no comment. I am however rather surprised, that Dr. Faraday, whose papers so ofter appear in another publication, the principle of which are for the purpose of vindicating the merits of

himself and others, should have permitted the above to pass unnoticed, and ventured to claim an invention which so obviously belongs to another.

I am, Sir, your obedient servant,

ELECTRICUS.

N. B. The following queries, if of sufficient importance, I should feel obliged by your inserting with a view to elicit answers to them from some of your correspondents.

1st. During the action of a voltaic current through the compound helix, of what character is the electricity developed in the secondary current?

2d. Can any reason be assigned why the magnetic lines should, by their proximity to an unelectrified or quiescent wire, create or communicate to the, at present, unrepulsive particles of electricity a repulsive character, so that they expand and form an electric atmosphere, extending outwards in every direction from the wire?

3d. Is the equilibrium of the electricity disturbed, for upon that depends the Franklin theory of any and every electrical effect?

4th. If, as in Mr. Sturgeon's theory, magnetism and electricity are principles, *sui generis,* and exist independent of each other in all bodies when in a quiescent state; by what action is either the one or the other made evident?

E.

To the Editor of the Annals of Electricity, &c.

Sir,

As many of your readers will probably have heard that the spark and other phenomena have been obtained from a thermo-electric pile, a description of that apparatus will probably be interesting.

Yours, &c.

THERMAL.

"There have been many trials to make thermo-electric piles that would operate like that admirable instrument for which we are indebted to the genius of Volta; but none have been crowned with that degree of success as has attended those of M. M. Nobili and Melloni. These two philosophers have constructed, in common, a thermo-electric pile, with which they have made some very interesting experiments on radiant heat; but we shall confine ourselves to a description of that made by M. Melloni.

"The pile which M. Melloni used was composed of fifty small bars of bismuth and antimony placed parallel, side by

side, forming one prismatic bundle F F, fig. 96, Plate XI, 30 millimetres long, and something less in diameter. The two terminal faces are blackened. The bars of bismuth, which succeed alternately those of antimony, are soldered at their extremities (to the latter metals), and separated from each other at every other part of their surfaces by some insulating substance (silk or paper). The first and last bars have each a copper wire which terminates at one of the pegs *c c*, of the same metal, passing through a piece of ivory fixed on the ring A. The space between this ring and the elements of the pile is filled with some insulating substance. The loose extremities of the two wires are connected with the ends of the wire of a multiplier, which indicates, by the motions of its needle, when the temperature of the furthest face of the pile is above or below that of the other." *Becquerel's Electricity.*

The piles which are now used in London are made of square plates of antimony and bismuth soldered together at the alternate edges, as shown in fig. 97, Plate XI; where the shaded pieces represent the antimony, and the unshaded pieces the bismuth. If the pile were used in the zigzag form, as in the figure, there would be no need of intervening insulating matter; but as the metals are usually packed closely together parallel to each other, it becomes necessary to place between the metals, intervening squares of card board, or thick paper, to prevent their touching one another. The terminal metals A and B, are furnished with wire, for the convenience of connexion.

We have to acknowledge the receipt of a pamphlet containing some excellent observations on mineral veins, with the theory of their formation, by R. Wear Fox, Esq, Falmouth. We have also received from the same gentleman a specimen of artificial copper ore, accompanied by a letter from which the following is an extract. EDIT.

"I send a specimen which has been changed, to some depth, by electrical agency, from the bisulphuret to the sulphuret of copper. This and other specimens, when stripped of their crust of sulphuret, have been found to be greatly reduced in weight, and yet W. J. Henwood has imagined that there has been no change, but merely decomposition. (See the Annals of Electricity, No. III, page 225). But if R. W. Fox's paper, in No, II, page 133, of the same valuable publication, be referred to, it will be seen that W. J. Henwood has not made his experiments as there described."

END OF THE FIRST VOLUME.

PLATE 1. ANNALS OF ELECTRICITY &c.

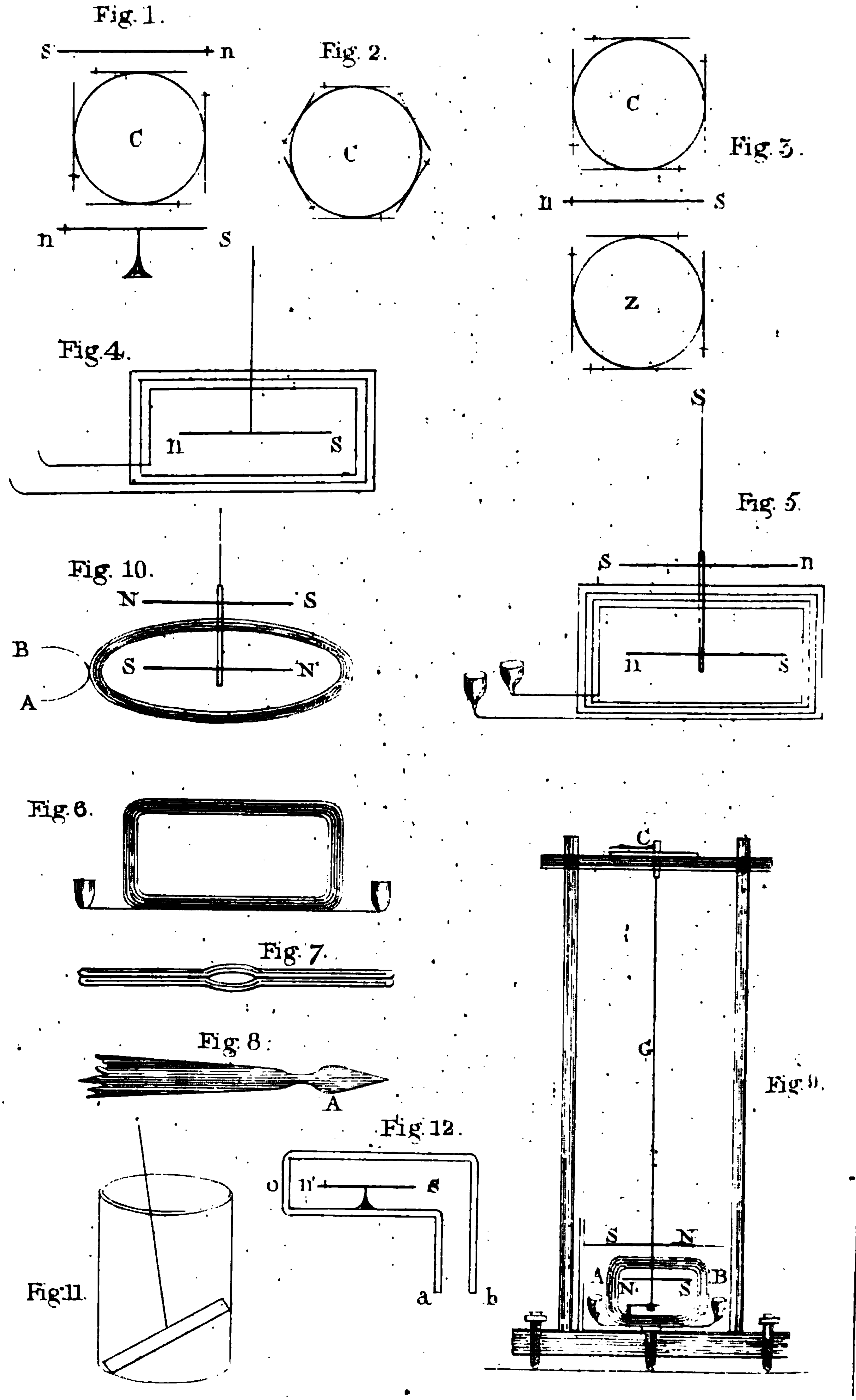


Fig 17

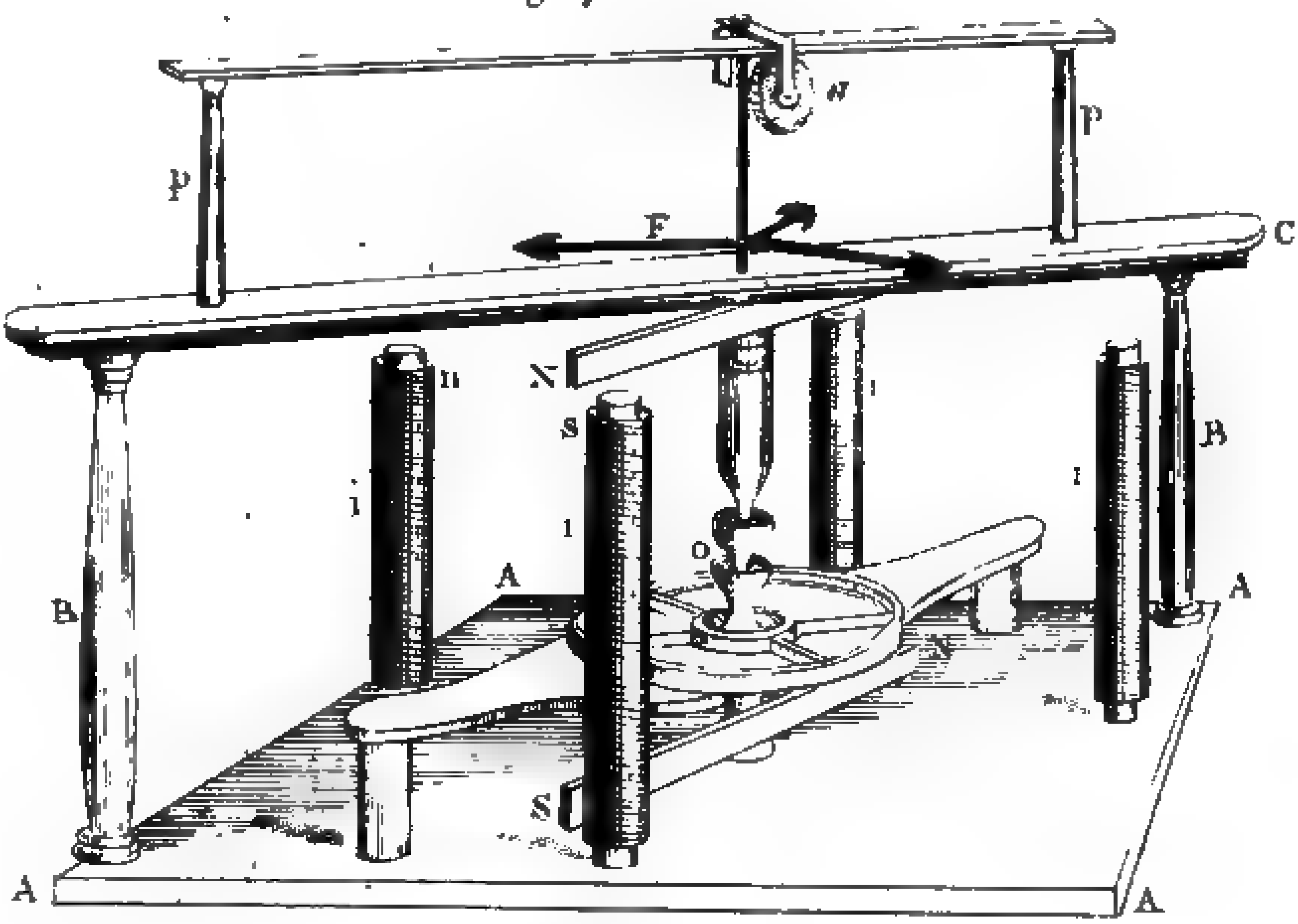
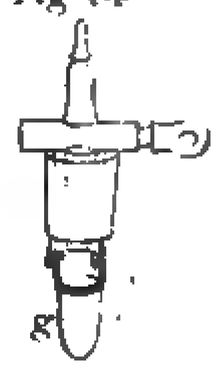


Fig 14



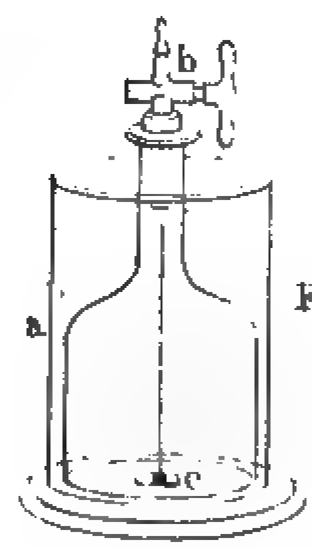


Fig 15.

Fig

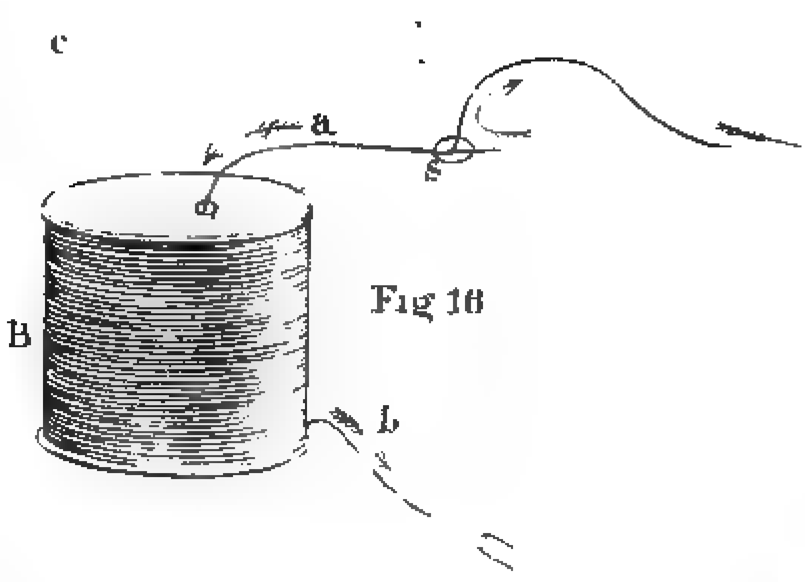


Fig 16

Fig 21.

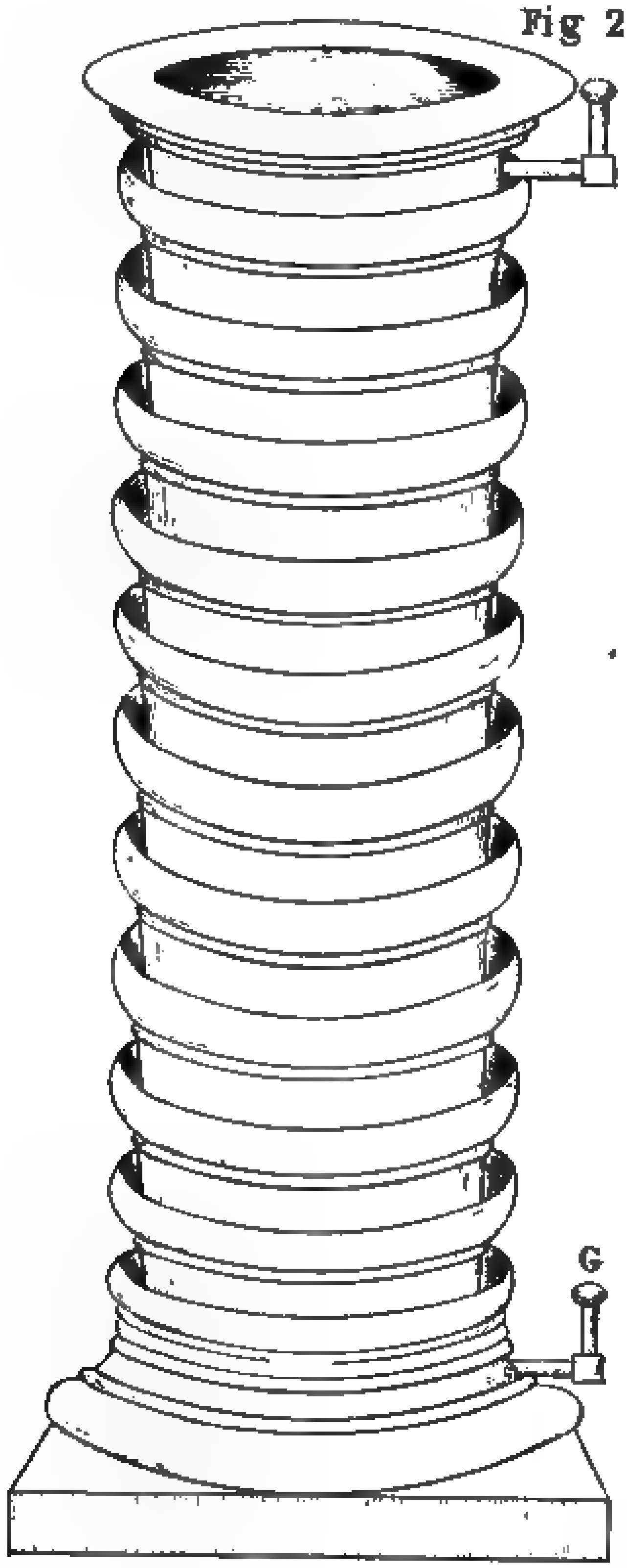


Fig. 20.

Fig 23

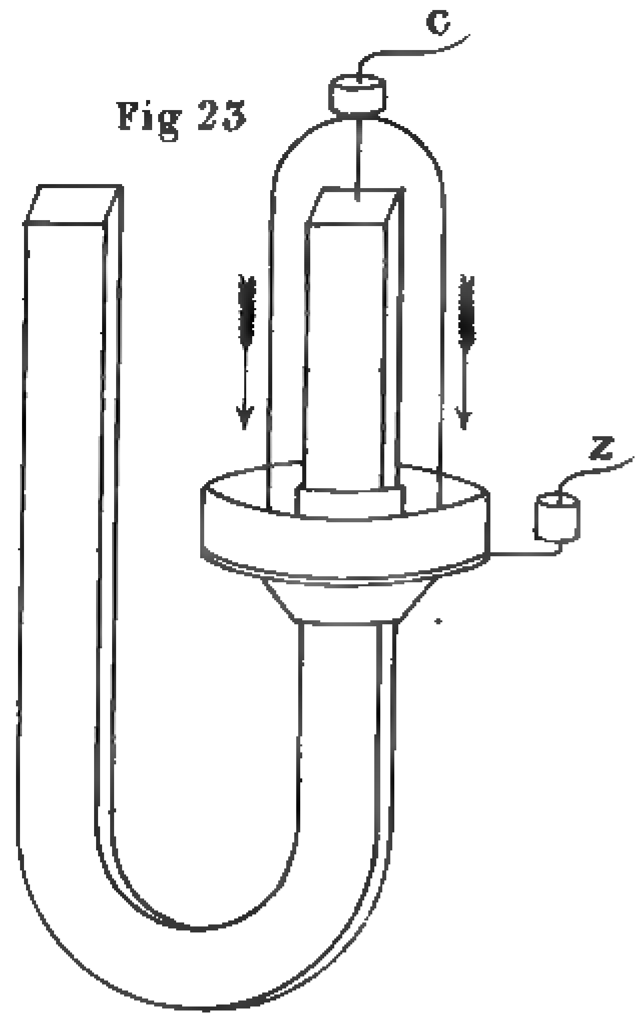


Fig 18.

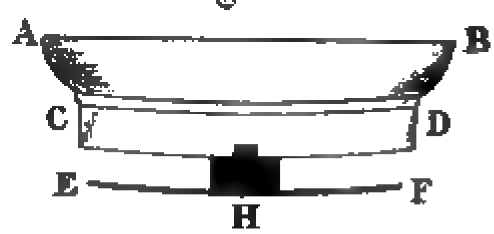


Fig 22.

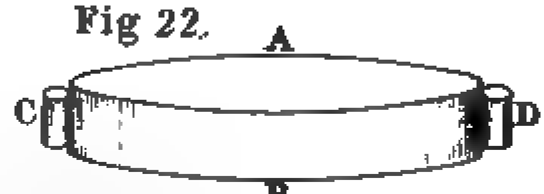


Fig. 19.

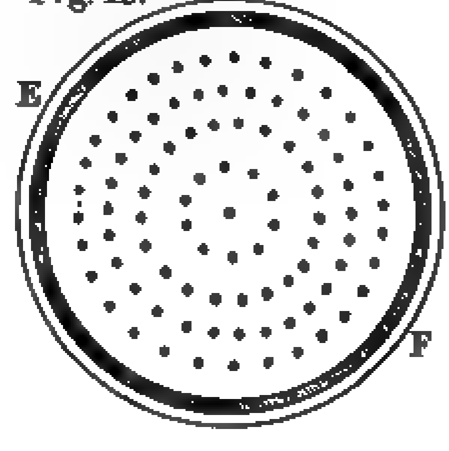


Fig 24

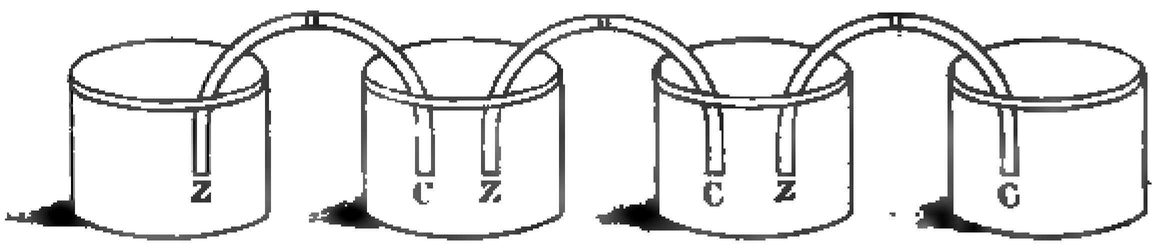


Fig 25

Fig 26

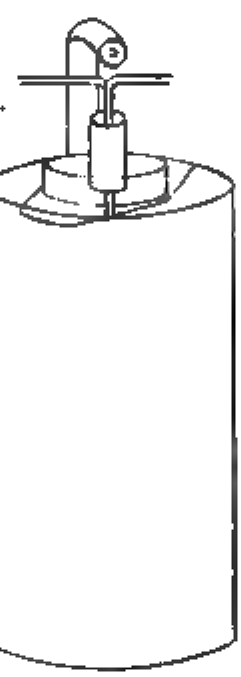


Fig.27.

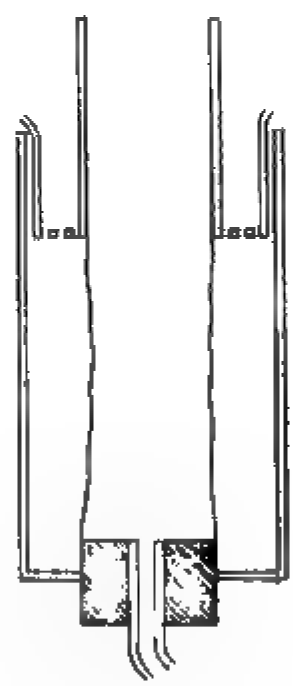


Fig 28

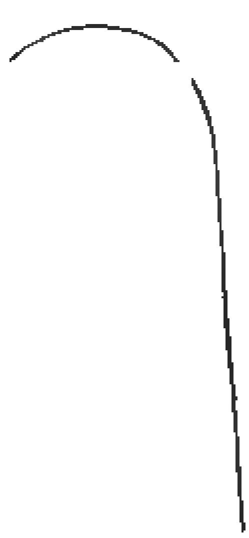


Fig 29

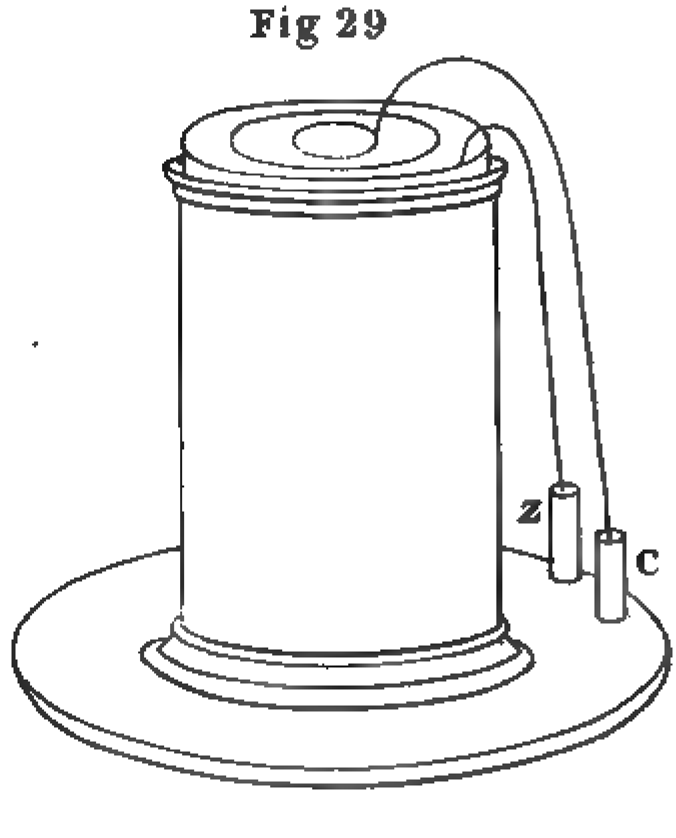


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Fig. 30.

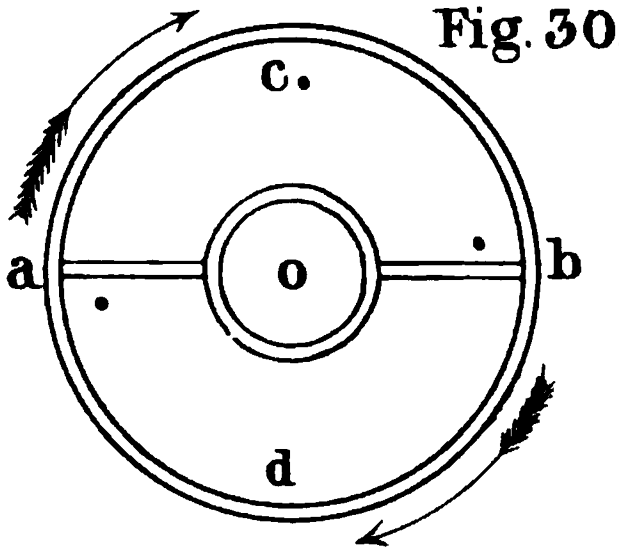

Fig. 31.

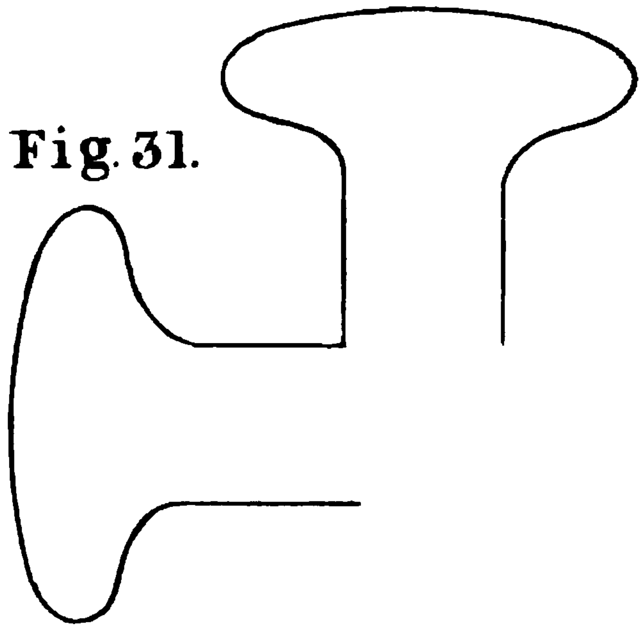

Fig. 32.

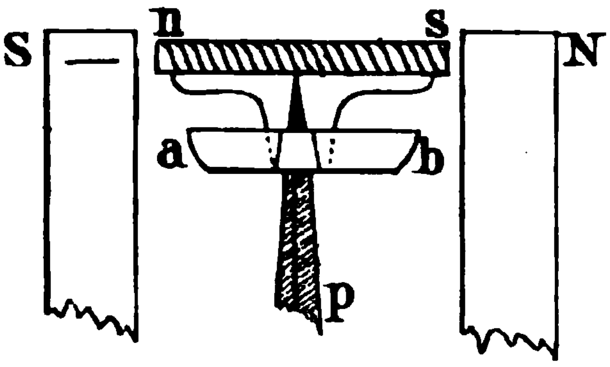

Fig. 33

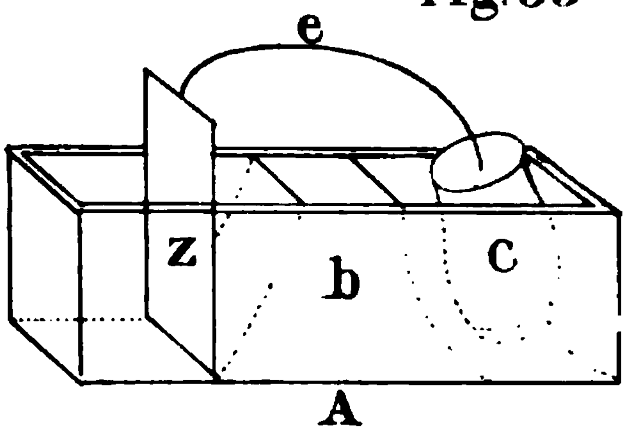

Fig. 34.

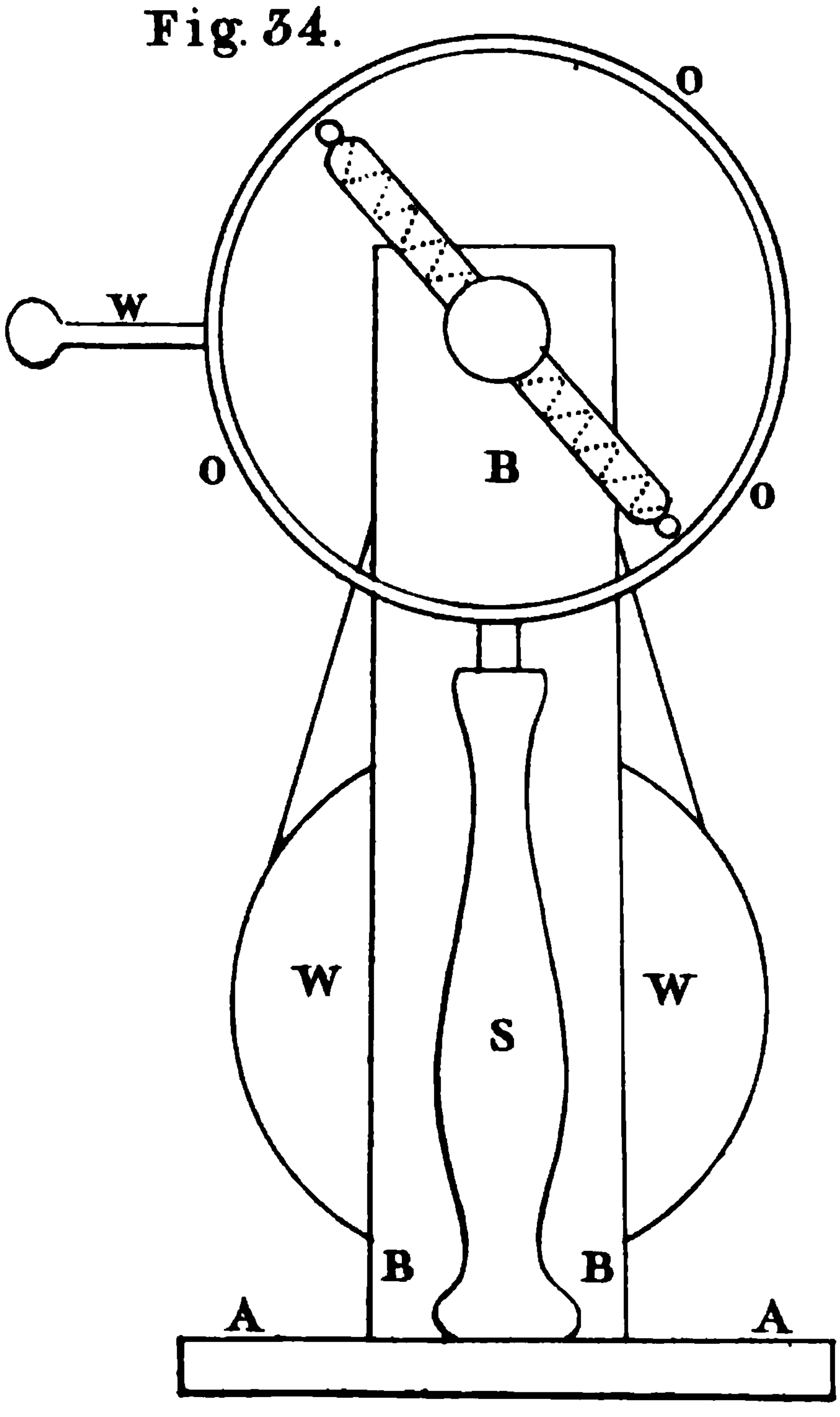

Fig. 35.

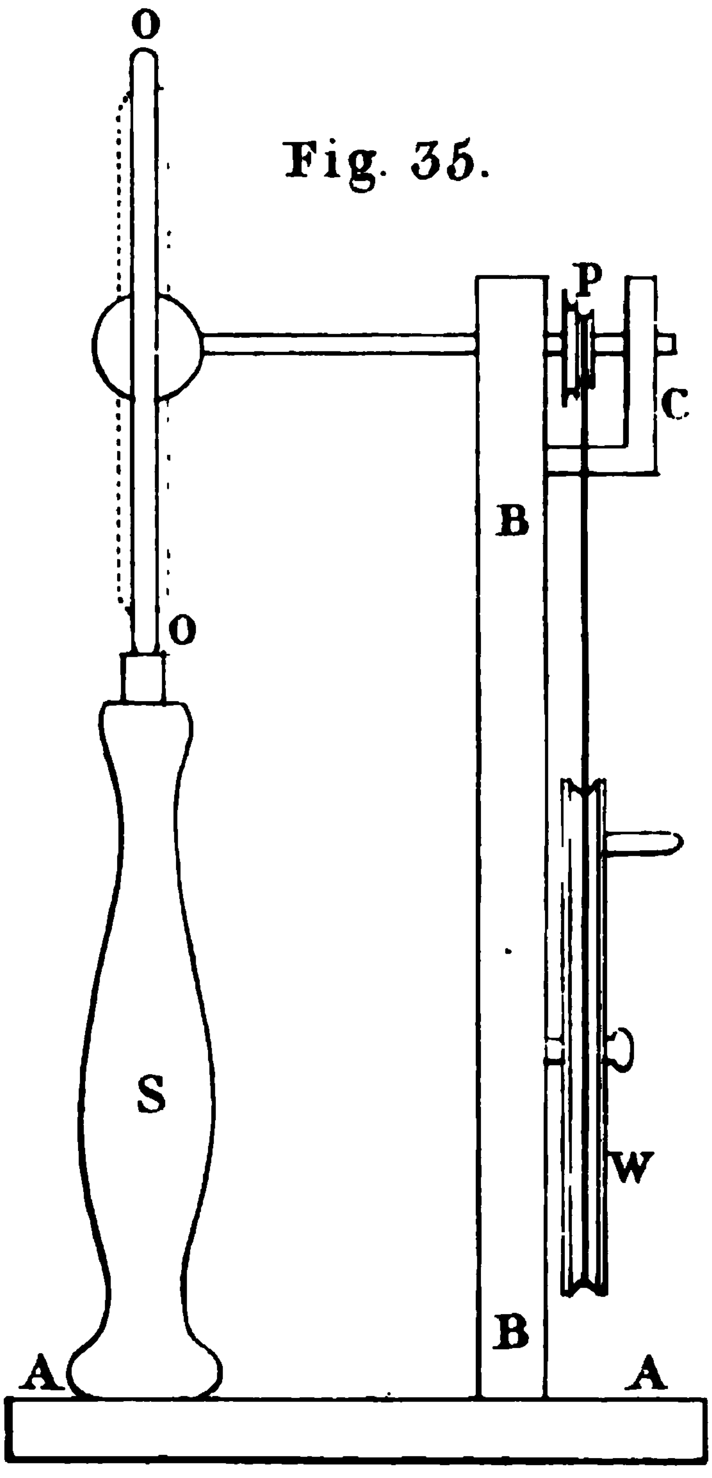

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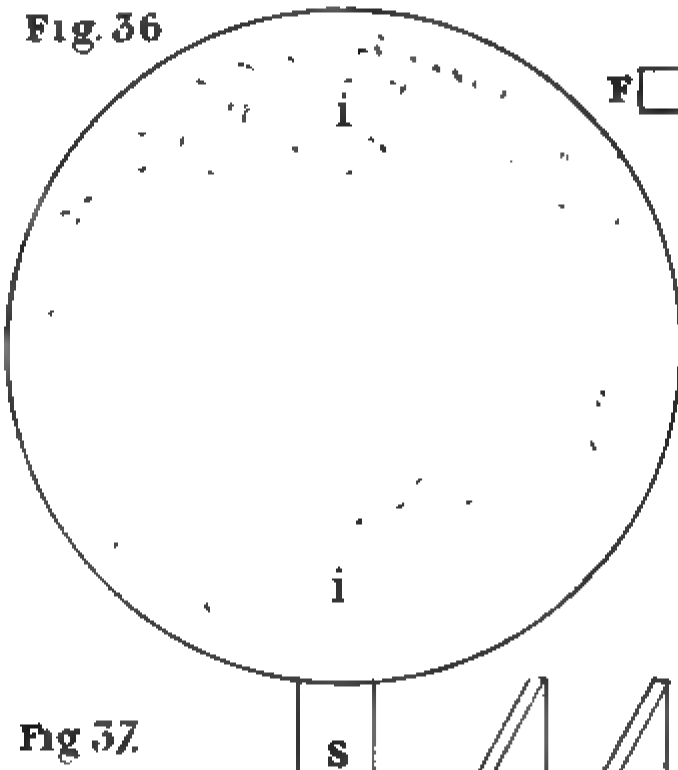
PLATE VI.

Fig. 36



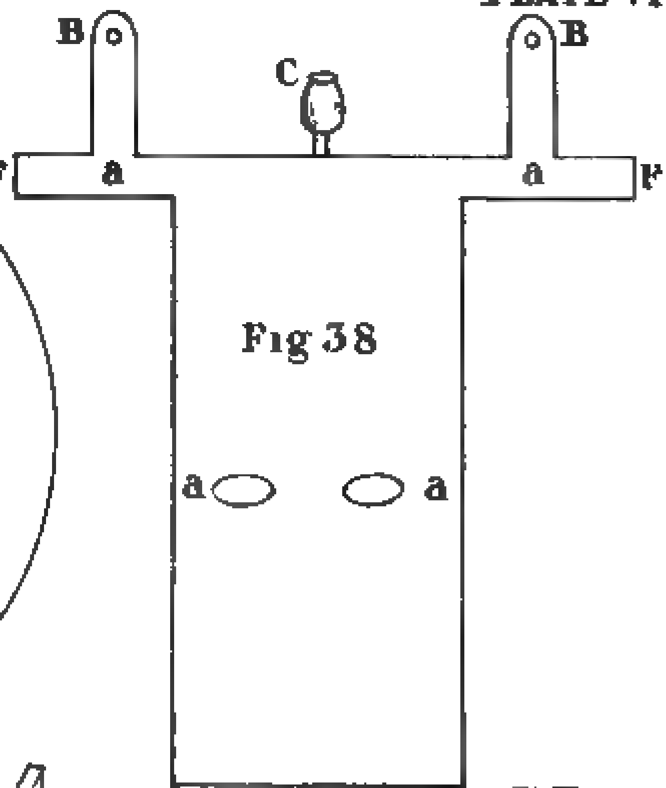


Fig 38

Fig 37.

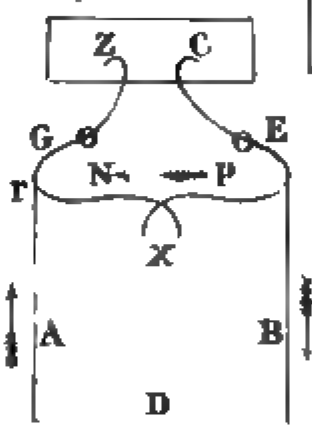


Fig 39

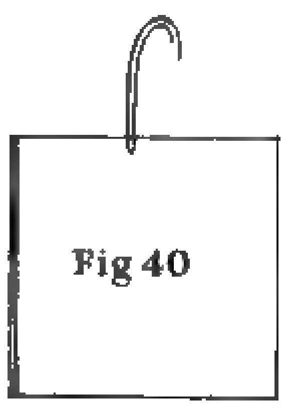


Fig 40

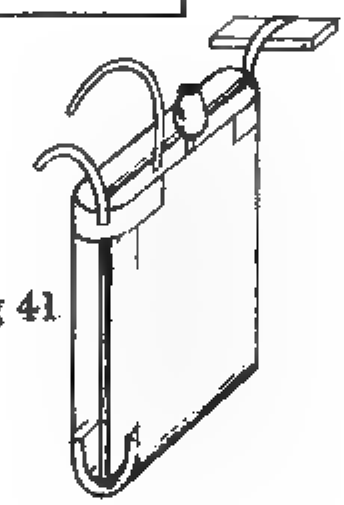
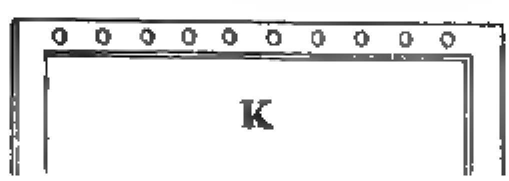


Fig 41



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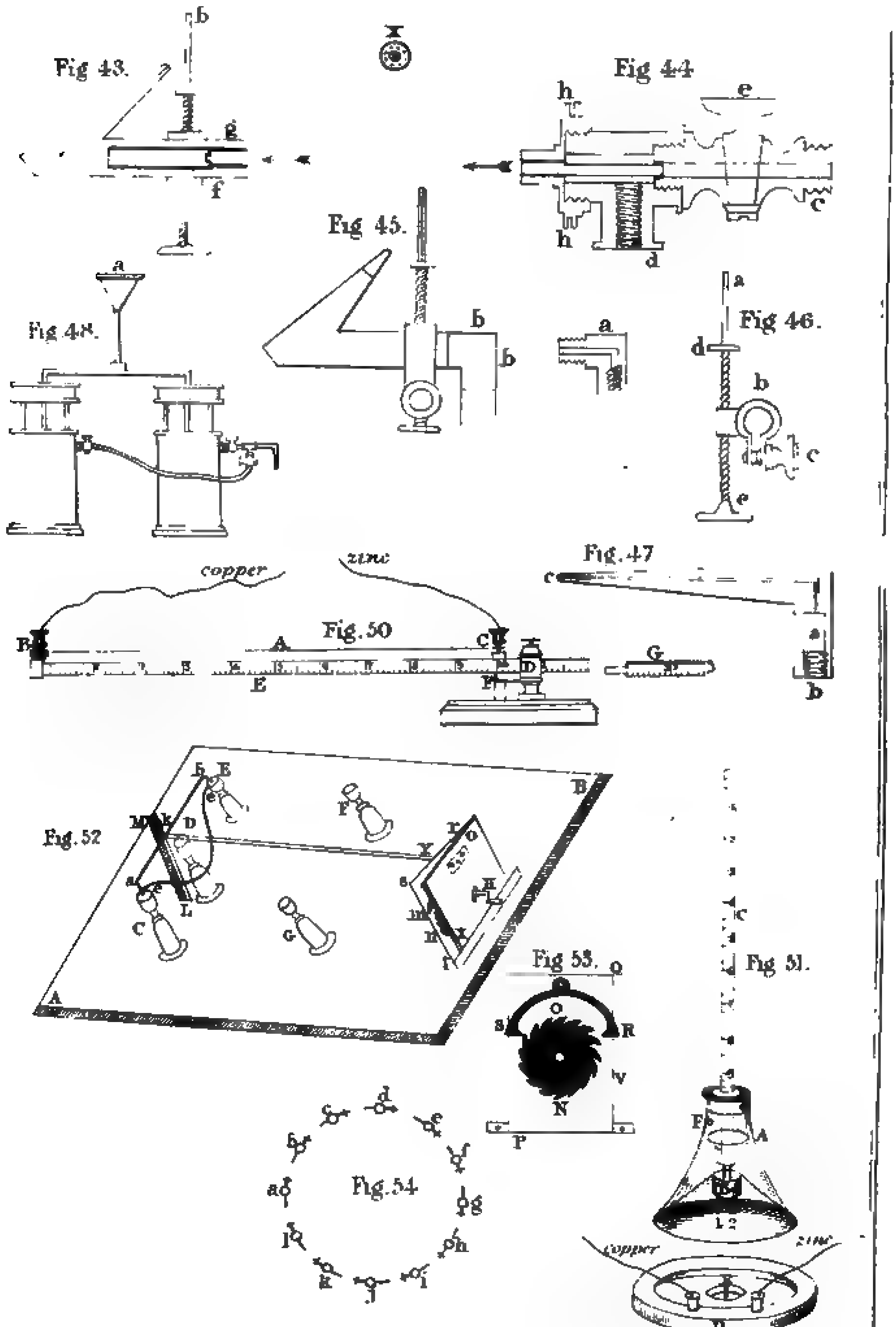

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Fig. 55.

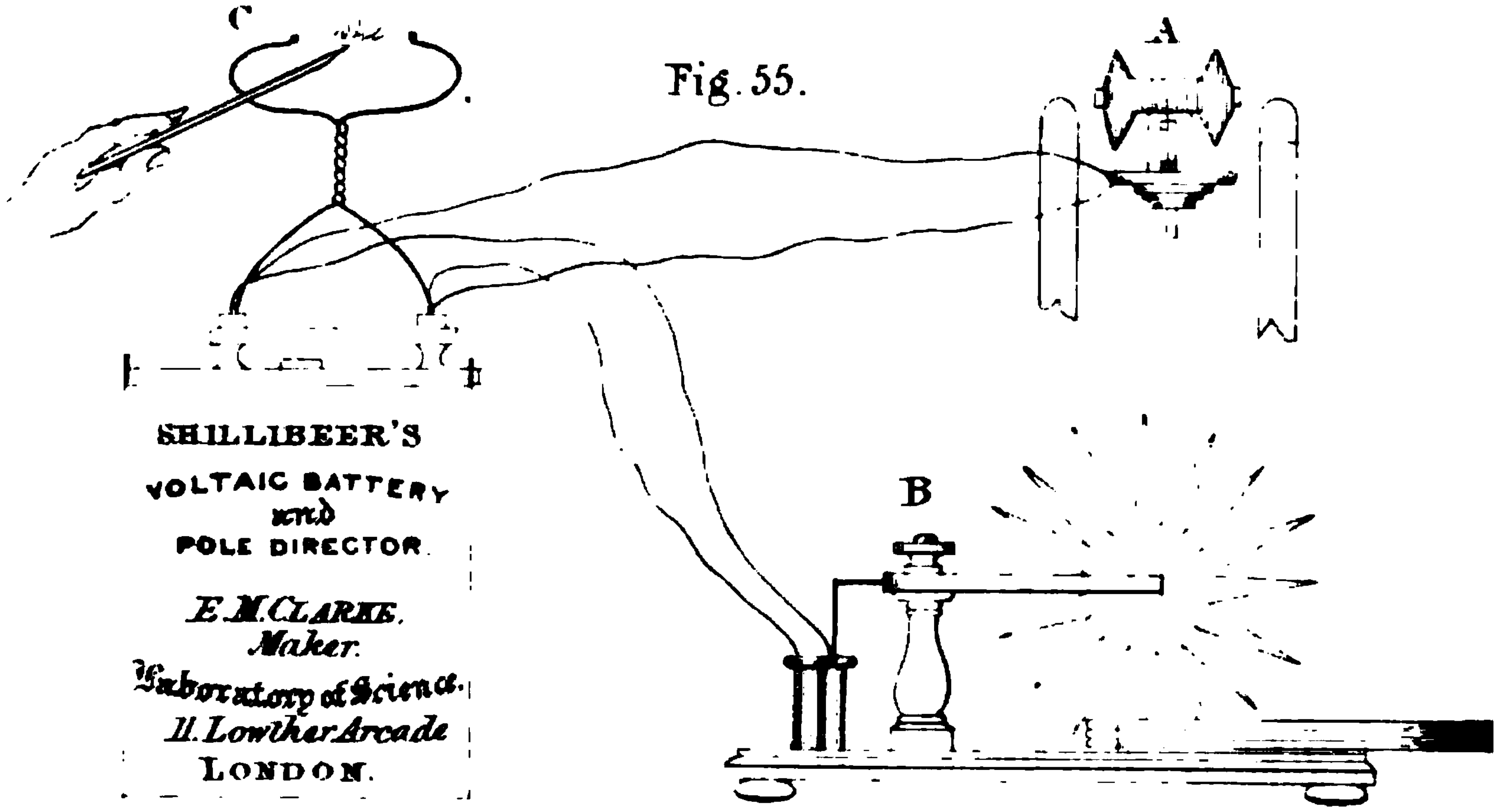


Fig. 56.

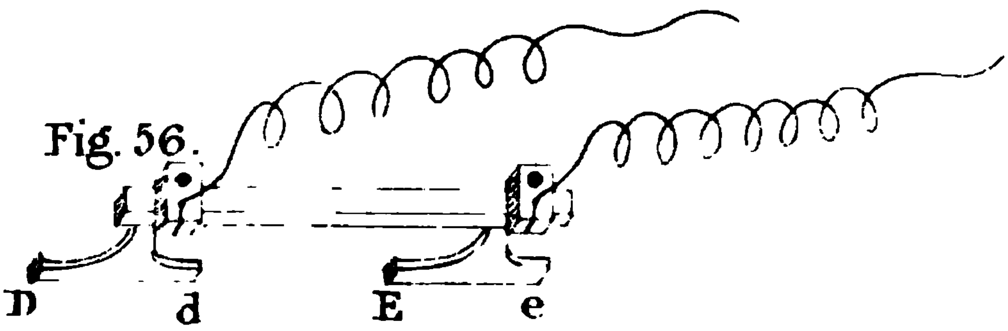


Fig. 57.

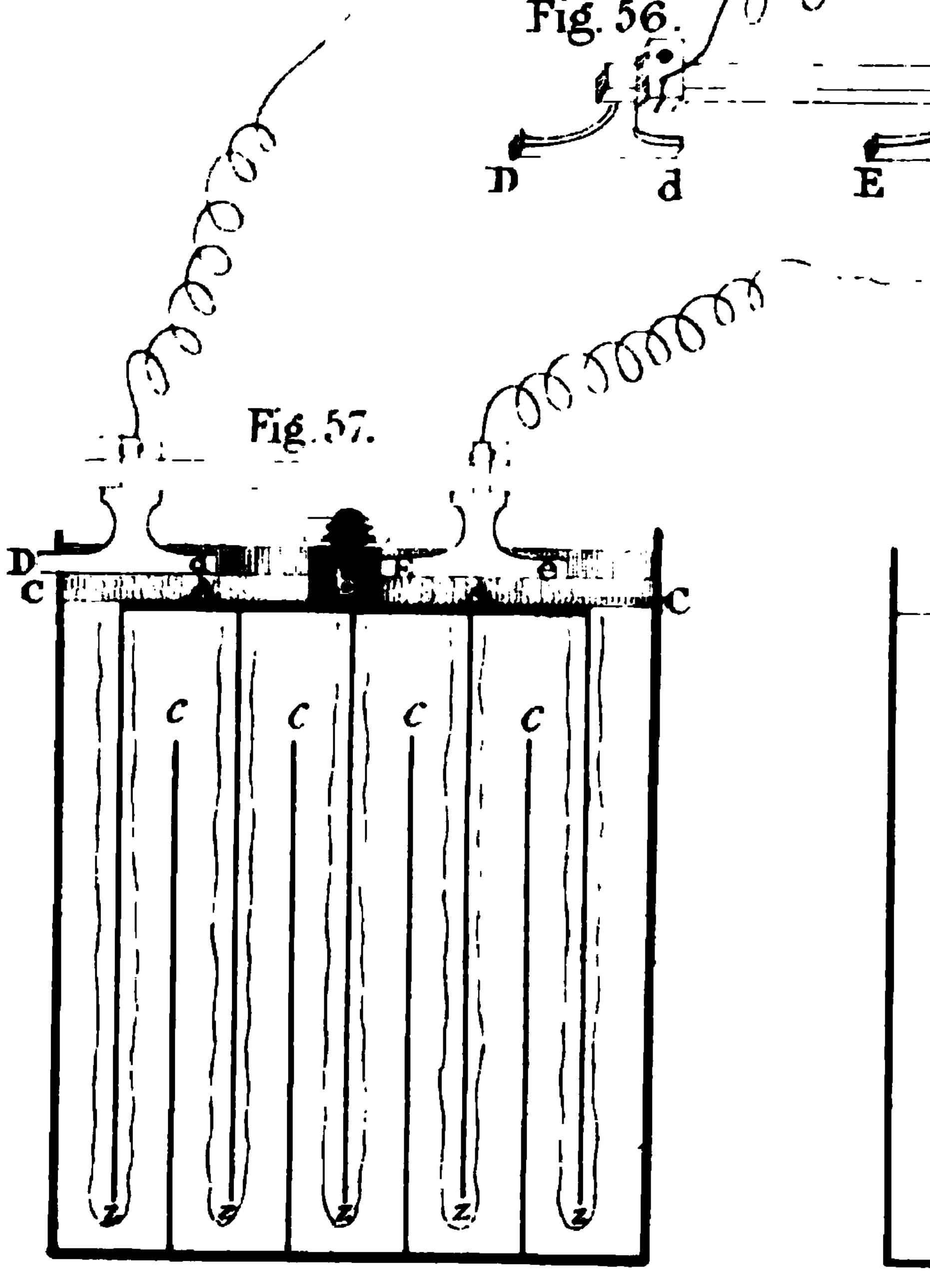
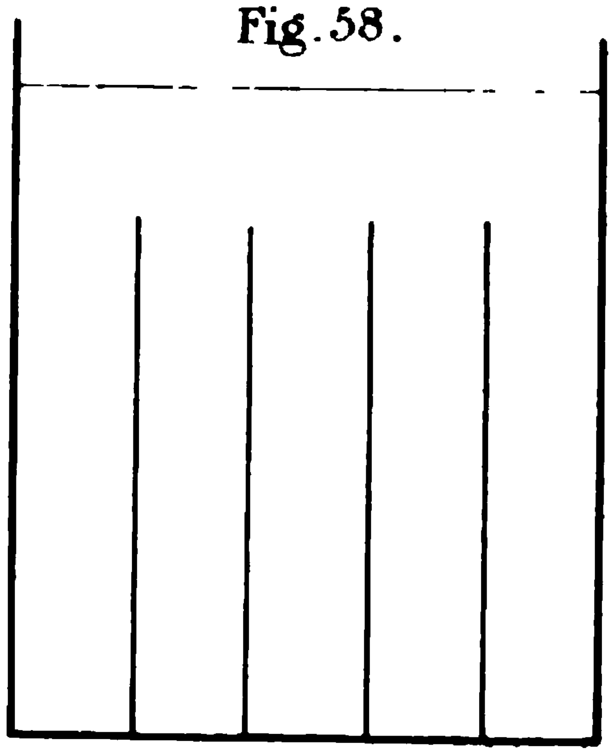


Fig. 58.



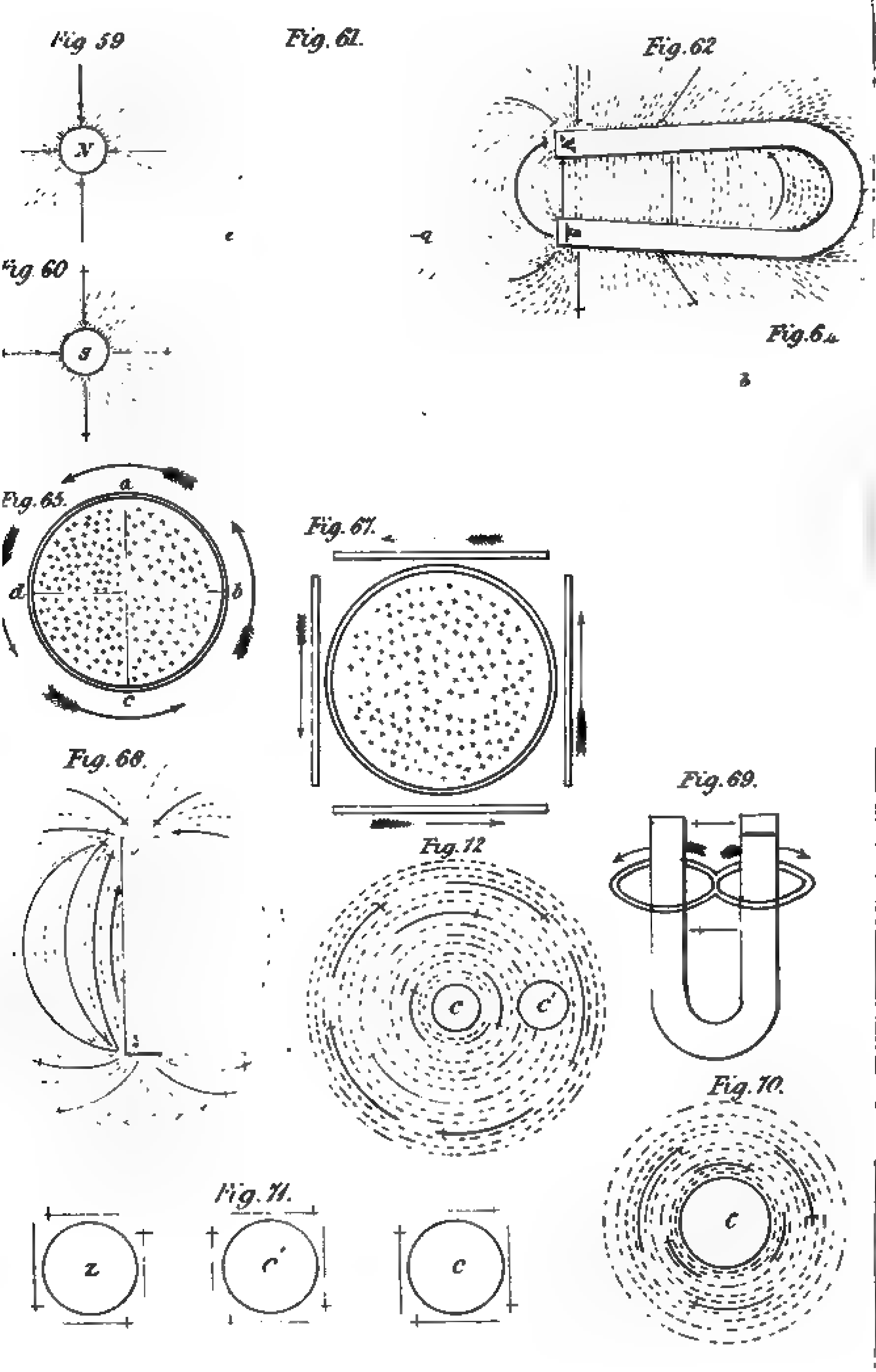


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Fig. 73.

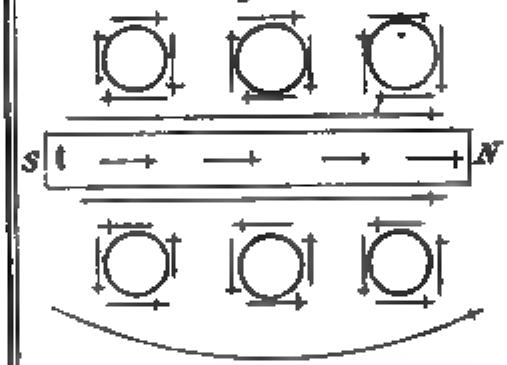


Fig 74.

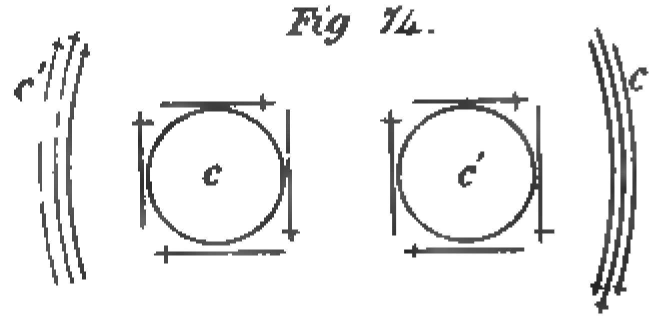


Fig. 81.

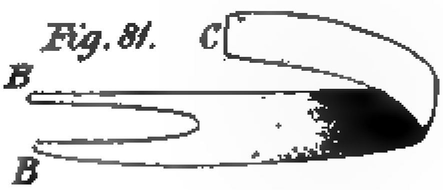
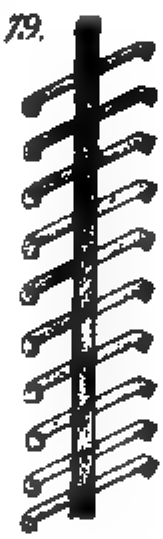


Fig. 80.

Fig. 78.

Fig. 76.

Fig. 79.



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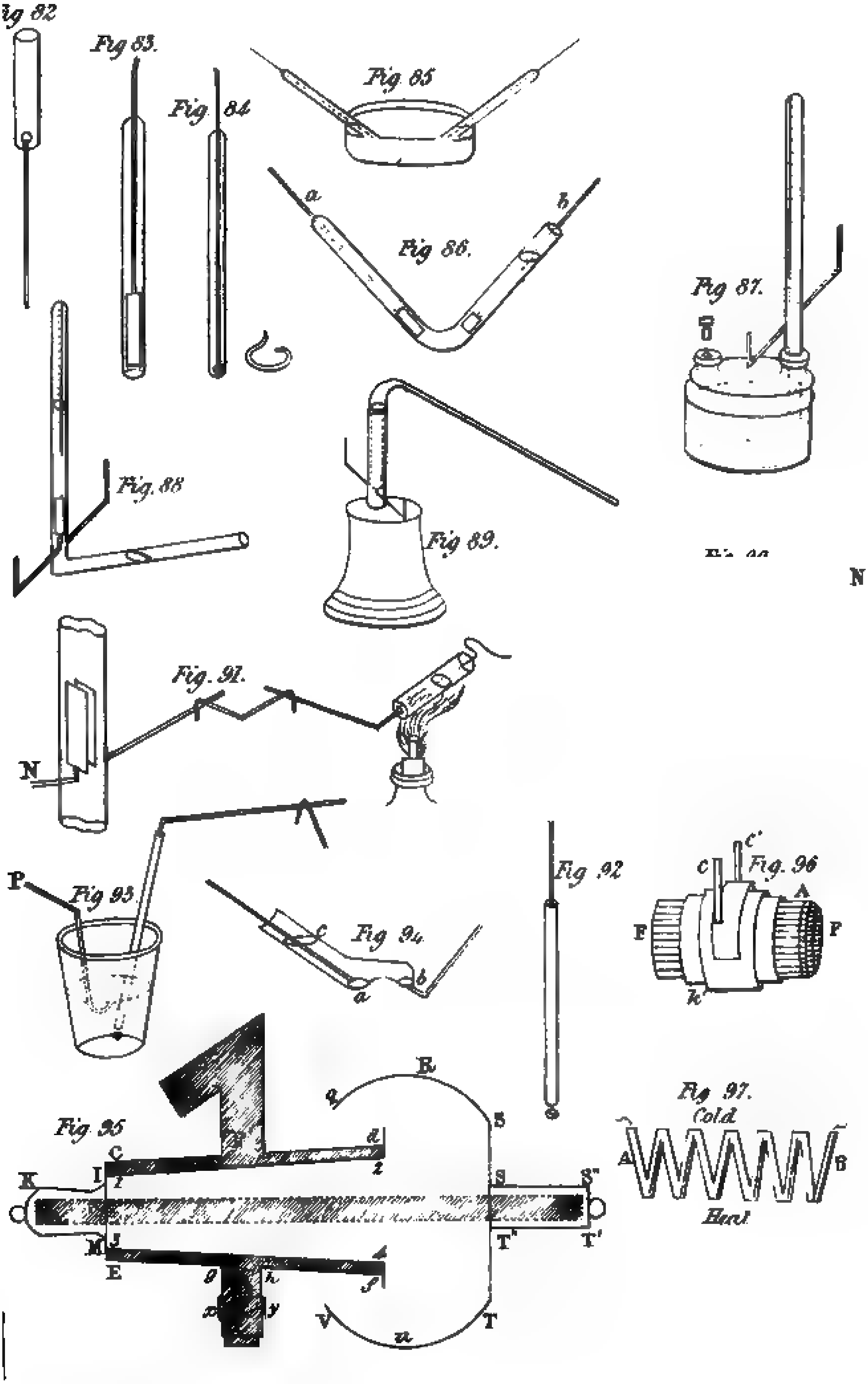

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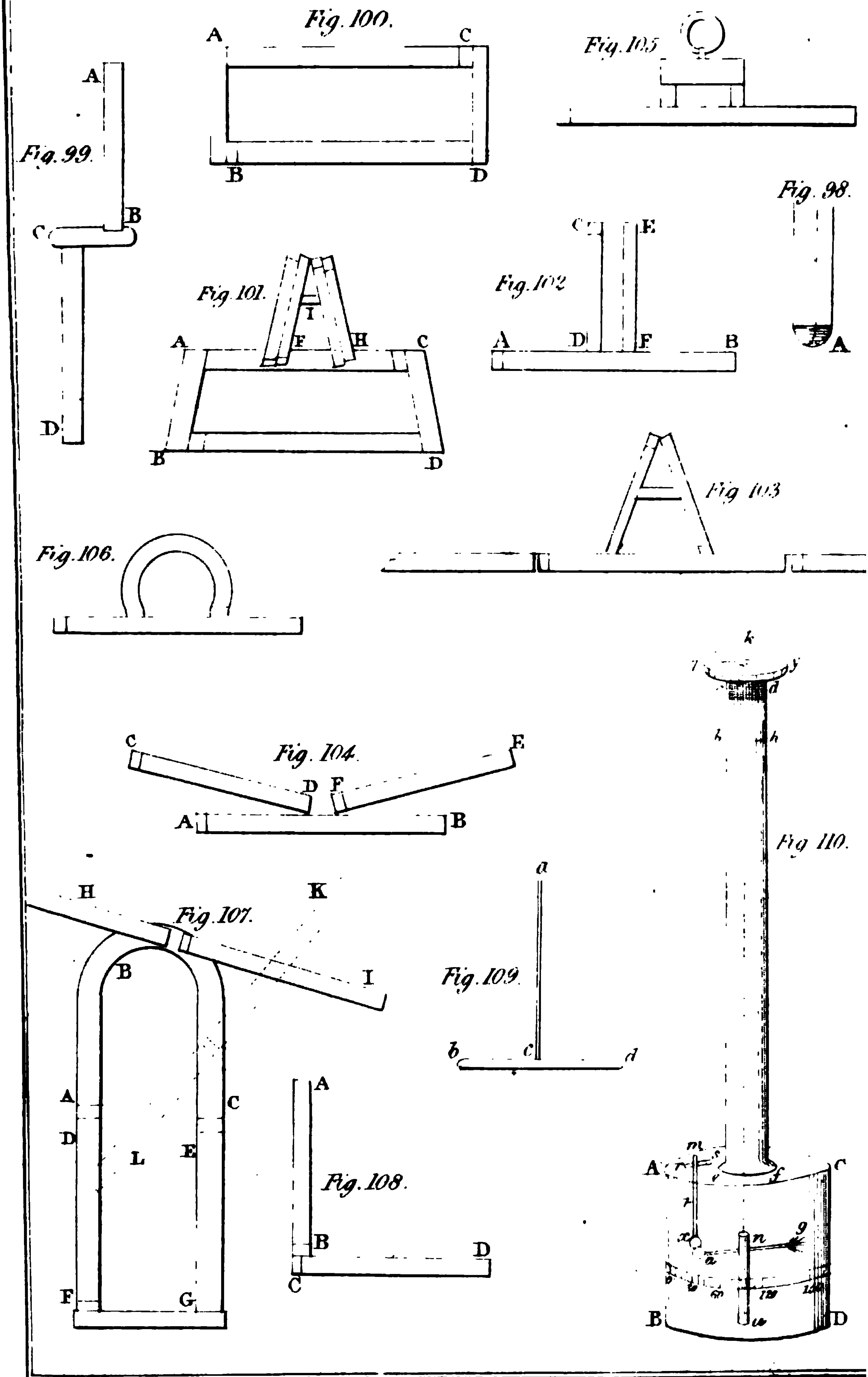

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Fig 111

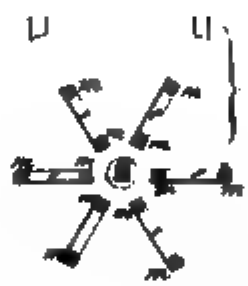
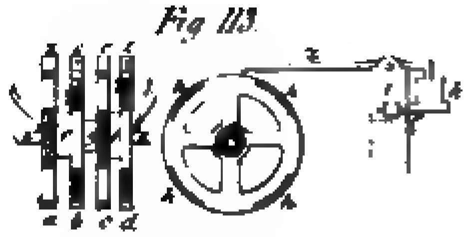


Fig 112

Fig 113



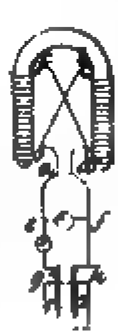


Fig 114

Fig 115

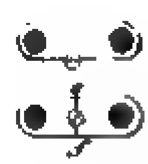


Fig 116

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PLATE XIV.

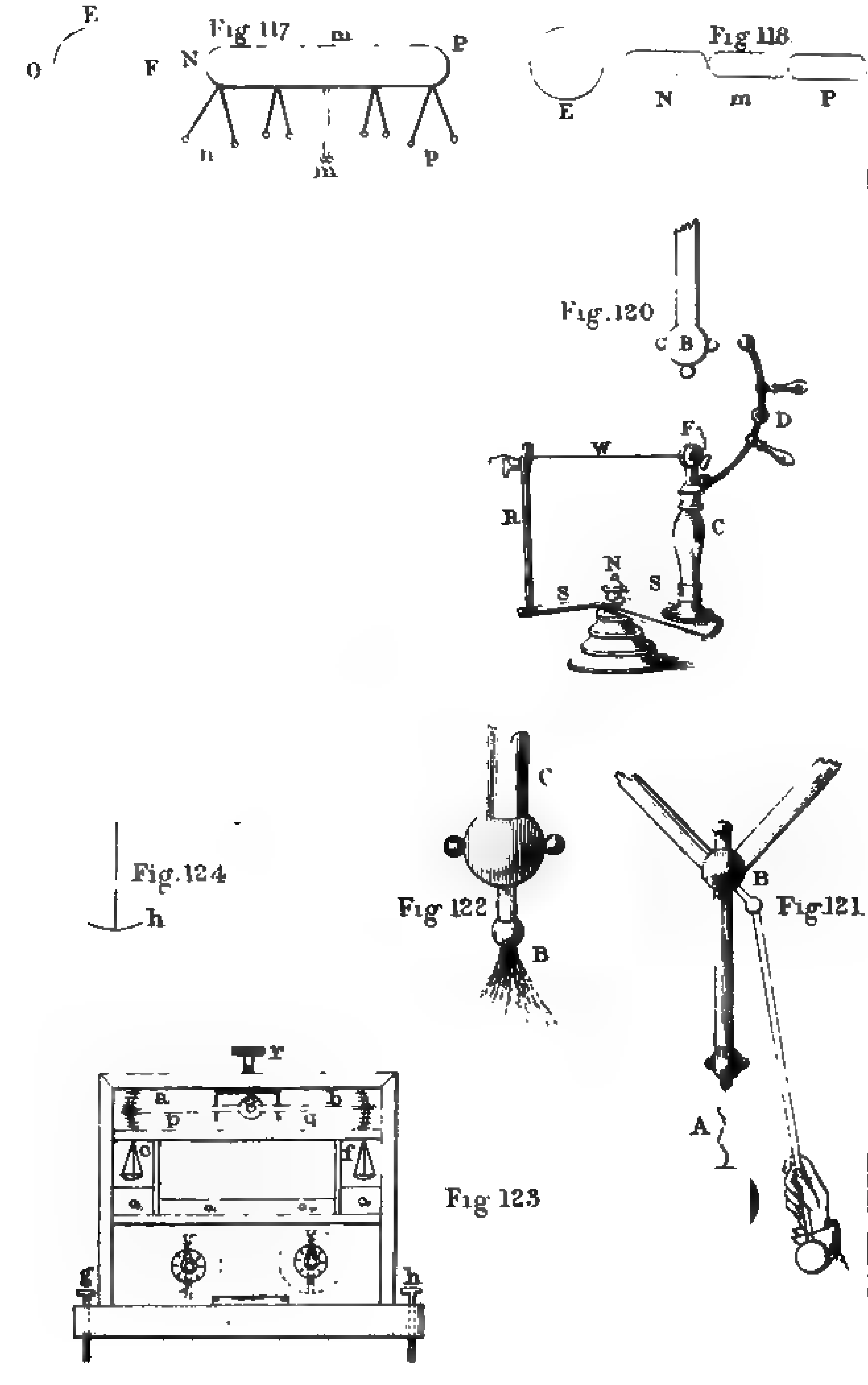
Fig 117
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Fig 118
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Fig. 120
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Fig. 124
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Fig 122
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Fig 121
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Fig 123

PLATE XV.

Fig 126

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Fig 128

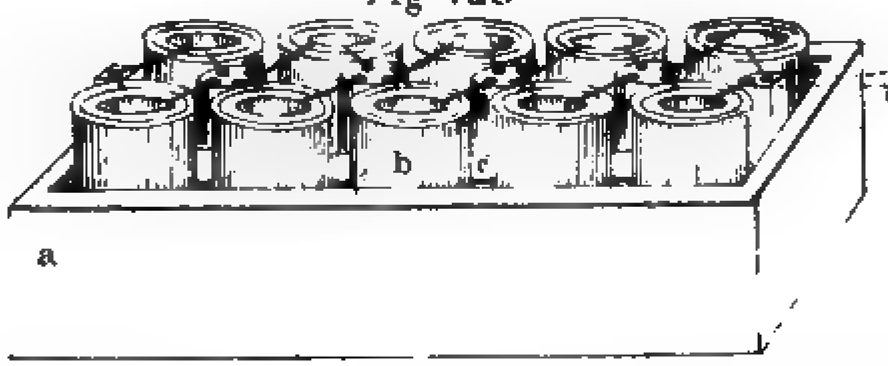


Fig 129

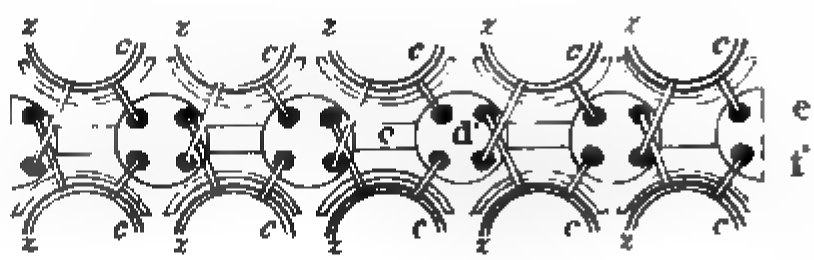
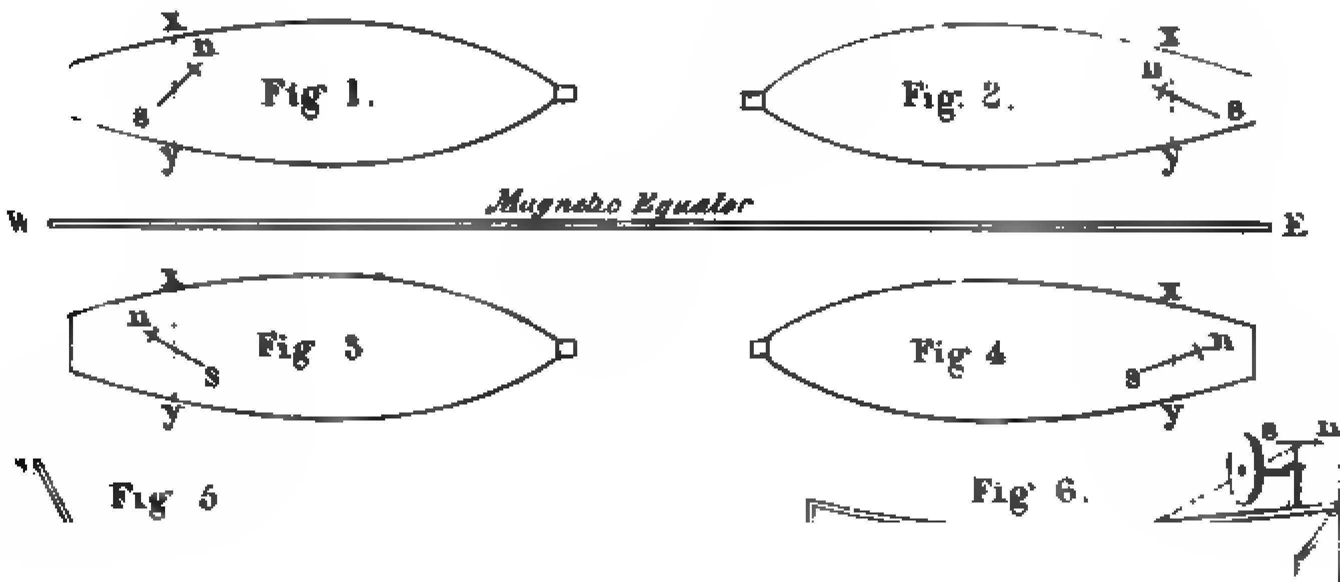


Fig 130


ANNALS OF ELECTRICITY, &c.

PLATE I Vol 3



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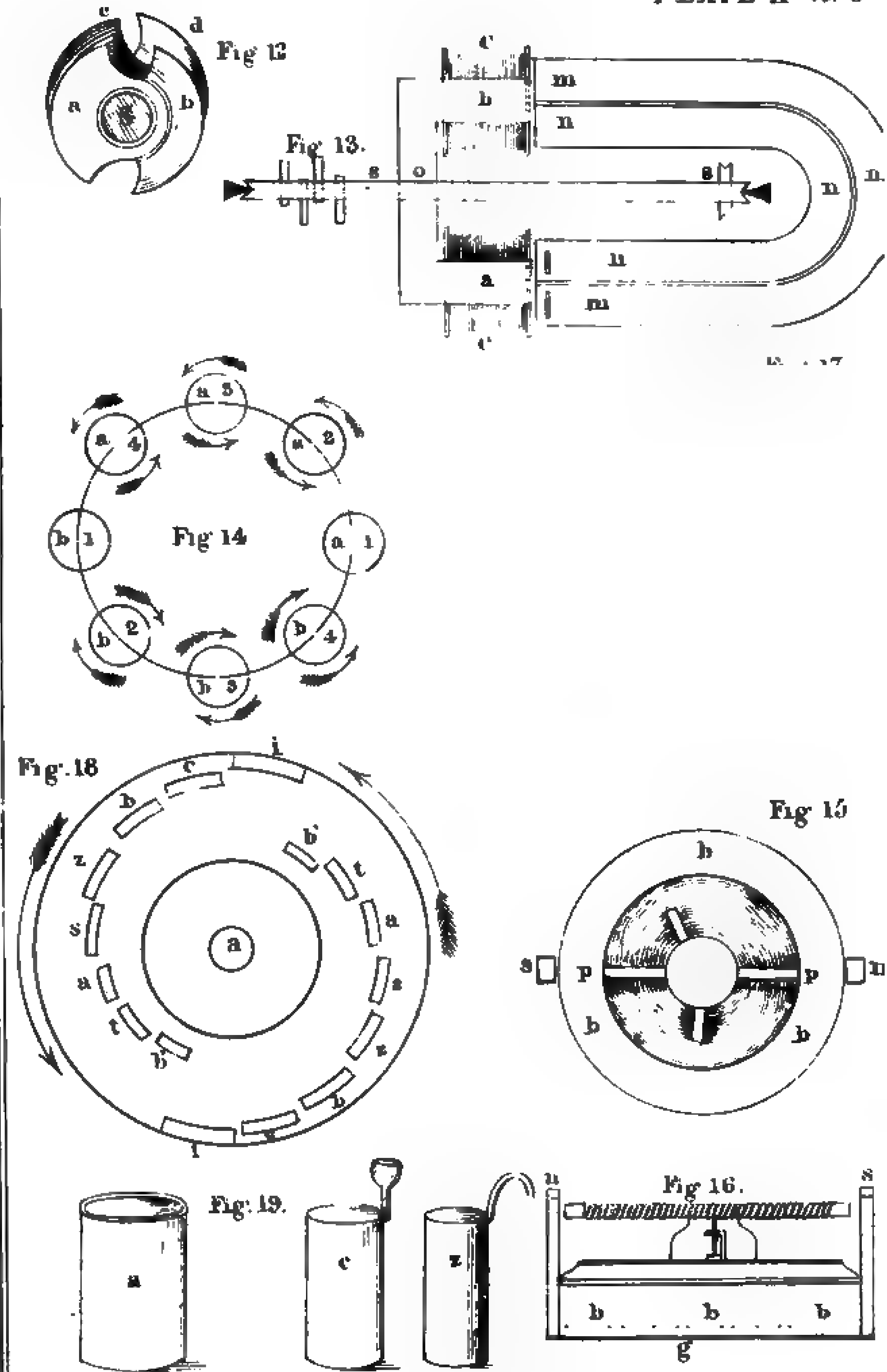
ANNALS OF ELECTRICITY, &c

PLATE II Vol 3



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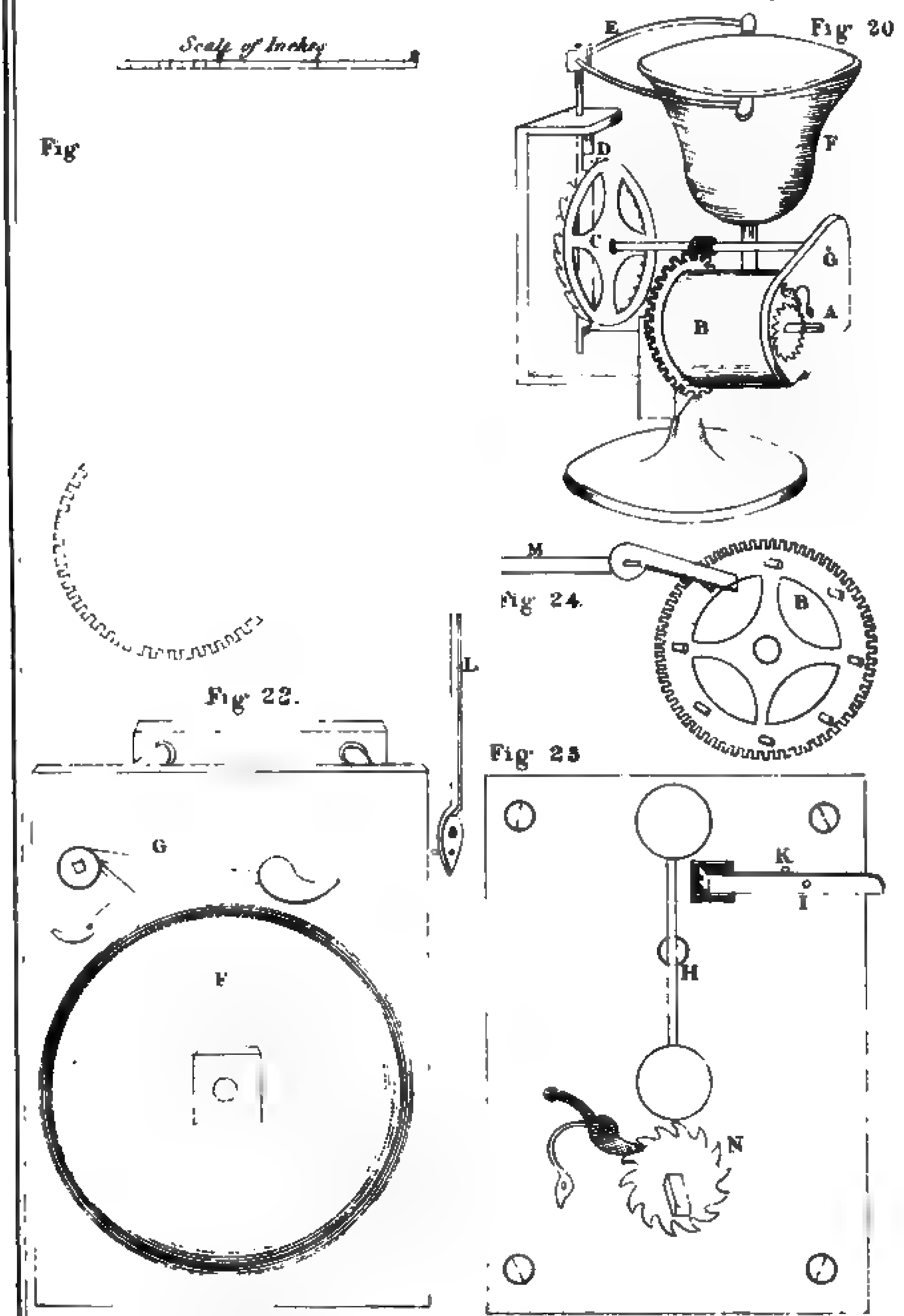
ANNALS OF ELECTRICITY, &c.

PLATE III.



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TO CORRESPONDENTS AND READER .

No. 3 of these Annals will appear on the 1st of April, 1837, and will be adorned by two or more Lithographic Plates illustrative of experiments and apparatus; and will contain amongst other articles:

The art of making artificial magnets.

An inquiry into the attributes of the galvanometer (resumed.)

On the production of electric shocks from a single pair of voltaic plates, by Dr. Faraday, *(continued)* with remarks.

Dr. Callan's galvanic battery.

A paper by W. Maugham, Esq. Chemical Lecturer, at the Adelaide Gallery, &c. &c.

The promised papers of Mr. Tomlinson, and of Mr. Marsh if they arrive in time, will also appear in our next number.

We have to acknowledge the receipt of the Dublin Morning Register, and Evening Freeman. The Brighton Herald, and Wigan Gazette. To the Editor of the latter Journal, we beg to return our sincere thanks for the very handsome manner in which he has been pleased to notice our labours.

"THE ANNALS OF ELECTRICITY, MAGNETISM, AND CHEMISTRY.—

" A new periodical under the above title has just appeared, edited by Mr. Sturgeon, Lecturer, on experimental Philosophy at the East India Company's Seminary, Addiscombe, and from the knowledge we have of the talents possessed by Mr. Sturgeon, we can safely predict that it will be a valuable work to the lovers of science in particular, and to the public in general. We have never spent more agreeable evenings than when listening to the lectures of this clever and persevering gentleman, nor have we ever been more astonished than on witnessing his experiments in galvanism and electricity."—*Wigan Gazette.*

We have also to acknowledge the receipt of Mr. Mitchell's very polite letter, and hope to be enabled to answer his enquiries satisfactorily in our April number.

It is requested that all Communications for these "ANNALS" *may be addressed, post paid, to* MR. STURGEON, *Adelaide Gallery of Practical*

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TO CORRESPONDENTS AND READERS.

In consequence of the accumulation of matter in the Editor's hands, it has been found necessary to publish an *extra* number; which will appear on the first of MAY.

Nearly two sheets which had been put in type and proofs printed for the present number, have been laid aside till the next, in order to give place to our Correspondents' communications.

Papers from Professor Callan, Sir Richard Phillips, and Mr. Maugham, and some others we have received, will appear in our *May* number. It will also contain the theory of Magnetic Electricity and Electro-magnetism, with their application to some of the most complex problems these branches of physics have hitherto presented,

We are much obliged to Mr. Hunt for his valuable suggestions; they are in strict unison with our plan. Any communication from Mr. Hunt's friend will be thankfully received and duly attended to.

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E. M. Clarke takes this opportunity of respectfully intimating that he has made arrangements in his new establishment to form private classes for Instruction in Electricity in its various branches. Viz.; Frictional, Chemical, and Magnetical. In this undertaking he will be assisted by scientific friends.

Further particulars can be had personally, or by letters, post paid, addressed to E. M. Clarke, Laboratory of Science, 11, Lowther Arcade.

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TO CORRESPONDENTS AND READERS.

Number 5 of the "Annals" will appear on the first day of July, and will contain, amongst other matters, the remaining part of Dr. Faraday's seventh series of experimental researches, &c., with remarks.

An Enquiry into the attributes of the Galvanometer resumed and concluded.

The method of making artificial magnets, and some other matter which has long been promised, will also appear in the 5th number.

Mr. Maugham's paper, which was promised for the present number, has been withdrawn for revisal. It is promised for our next.

Our Correspondents are requested to send in their communications as early as possible.

Plate 11, referred to in Dr. Faraday's paper will appear in our next number.

---

dent that the matter treated of may be more fully developed than if the whole were embraced in one short course. E. M. Clarke takes this oppor- of returning his grateful acknowledgments to those Ladies and Gentlemen who have honoured him with their presence during

Mr. Sturgeon's Lectures

ON

ELECTRO-MAGNETISM

AND

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and to state, that in consequence of the great number of persons which he was compelled to disappoint on account of the class being limited to 40 persons, that course will be repeated. Half the Class is now formed.

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NOTICE TO CORRESPONDENTS.

Mr. Clibborn's and Dr. Simon's papers will appear in our next number.

A description of a Magnetic Electrical Machine, (without iron armature) will also appear in our next.

We are promised, by their authors, some of the papers, in their original forms, which were read at the Liverpool Meeting of the British Association for the promotion of science. All those that arrive in time will appear in our next number.

The second volume of the "Annals" will commence on the first of January, 1838, and will be continued in monthly numbers.

The title page and index for the first volume will be given with our next number.

---

ratus of the best manufacture, and on very moderate terms at his warehouse as above. Catalogues gratis.

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